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LECTURES
ON THE
THEORY AND PRACTICE OF MEDICINE;

NOW IN COURSE OF DELIVERY AT THE THEATRE OF ANATOMY AND MEDICINE, WEBB-STREET, SOUTHWARK.

BY

MARSHALL HALL, M.D., F.R.S. L. & E.
&c., &c.

MELANOSIS.—*Rarity and difficulty of detecting the disease; its treatment unknown. Description of the morbid secretion; various forms in which it is laid down; is most frequent in advanced age; symptoms and treatment.*—ENCEPHALOSIS; SCIRRHUS. *Appearances and forms. The parts most liable to be affected. The symptoms, course, and termination. The local effects of encephalosis. Scirrhus acts often by pressure on surrounding parts. Œsophageal vomiting produced by scirrhus. Its effects sometimes taken for aneurysm. Cancer and its terminations.*

GENTLEMEN:—Leaving much to be said on tubercle until I come to treat of *phthisis*, of *tuberculous disease of the mesentery*, &c., I pass on to *melanosis*.

ON MELANOSIS.

The designation *melanosis* was given by Laennec to a peculiar disease, which has recently greatly engrossed pathologists, but which is, in reality, of little importance to the practitioner:—1. It is a rare disease; 2. It is, in general, indetectible during life; and 3. If detected, it suggests no indications for the treatment. I will devote to it precisely that degree of attention which it appears to deserve, comparatively with the other subjects of these lectures.

Melanosis consists generally of a deposit,—a *secretion*—of a dark colour, varying in shade from brown and other hues to deep black. It stains paper like Indian ink. It seems to consist principally of the colouring principle of the blood greatly modified.

Like tubercle, melanosis appears, as I have stated, to be a morbid secretion. It is inorganised, and incapable, of course, of being injected. It exists in masses, or patches, the forms of which are greatly modified by the texture and forms of the part in which it is deposited; it exists, therefore, sometimes in rounded portions, as in the lung, the liver; sometimes in lamellated or stellated portions, as in the intestines, or the surface of the serous membranes, &c. It is—

1. *Isolated.*
2. *Agglomerated, and of various size and form.*
3. *Infiltrated.*
4. *Encysted.*

It is usually of uniform texture. It is occasionally seen, in cysts and in the natural cavities, in the *liquid* form.

Like tubercle, the *size* of melanosis varies from that of a millet seed, to that of a pea, of an egg, of an apple; and its *forms*, from the round, or oval, to the lobulated, are induced by the intersection of portions of cellular membrane,—and to the perfectly irregular, induced by the various coalescence of minor masses.

Melanosis rarely undergoes the process of softening and expulsion; it then, however, leaves an ulcer or cavity, which may enlarge or contract, and ultimately cicatrise.

Like tubercle, melanosis generally exists in several organs simultaneously. It is most frequently found in the *lungs*. It occurs occasionally in the *liver*. M. Andral observes that it has not been observed in the *brain;* it has, however, been both described and depicted by Lobstein, as occurring in the optic thalamus; and in the brain itself by Dr. Carswell. It occurs in the stomach and intestine; in the lymphatic glands; in the ovarium; in the subcutaneous cellular tissue; in the bones, &c.; and it occurs in masses of encephalosis or scirrhus.

Unlike tubercle, melanosis is most frequent in advanced age.

The existence of internal melanosis is sometimes revealed by the appearance of similar deposits under the skin, through

which the black colour is more or less distinctly or faintly discerned.

The *symptoms* of melanosis are very obscure; Laennec observes that there is no fever, but emaciation, and dropsy of the extremities, and even of the serous cavities.

Laennec observes,—"The subjects who have died of melanosis, under my observation, have presented no marked fever. The two cases given in the work of Bayle present the same result. If this character of melanosis be constant, as I am inclined to think, it may afford a diagnosis between this disease and tuberculous phthisis, which is constantly accompanied, in its whole course, by hectic fever, generally characterised by *two* exacerbations, of which one occurs in the middle of the day, the other in the night."

Of the remedies of melanosis nothing is known.

The subject of melanosis has been principally treated of by Laennec, Bayle, M. Breschet, M. Cruveilhier, M. Andral, Dr. Carswell, &c. M. Cruveilhier is of opinion that melanosis is formed in the capillary vessels, or minute veins; Lobstein has depicted the matter of melanosis within a small vein.

ON ENCEPHALOSIS, SCIRRHUS, &C.

Subjects of far greater *practical* interest than melanosis, are *encephalosis* and *scirrhus*, or *soft* and *hard cancer*.

Encephalosis, as its designation, encephaloid, imports, resembles the medullary portion of the brain, both in consistency and colour.

Scirrhus, on the other hand, as its etymology implies, is extremely hard, and of the consistency and colour of cartilage.

These two forms of disease, so apparently dissimilar, occur too frequently simultaneously, in the same organ, or in different organs, to be viewed as distinct diseases. It is probable, however, that they are not changed, or transformed into each other. They are rather variously mingled together.

Encephalosis is very vascular, and, if torn or cut, in the living subject, is very apt to bleed. It was designated by the term fungus hæmatodes, by the late Mr. Hey. It consists of a peculiar substance frequently interposed between the meshes of the cellular membrane, whence it obtains various lobular and other forms.

Scirrhus is less, or not at all vascular, and appears to consist, according to M. Andral, of the same cellular membrane, in a state of induration and hypertrophy.

These two forms, of the disease are homogeneous. *Intermediate* forms, however, consist of admixtures of these two substances in various manner and proportion. Scirrhus is frequently described as consisting of meshes, or cells, formed by hard, white, opaque bands, which contain, in the spaces formed by them, another of transparent and softer matter: it is true encephalosis and true scirrhus mingled together.

Encephalosis and scirrhus constantly tend to *enlargement*, to *ulceration*, to *multiplication*, to *diffusion*. It frequently happens, alas, that when an external fungus has been removed by the surgeon, the symptoms of the internal disease begin to manifest themselves.

This disease has a tendency to assume different forms in different organs. Thus, it is generally fungous in the encephalon, tuberous in the liver, hypertrophous in the stomach and rectum. This rule is by no means constant, however.

The parts most liable to this disease are the eye, the encephalon, the lungs, the liver, the stomach, the rectum, the kidney, the testis, the lymphatic glands, the bones; but by no means in the precise order in which I have here enumerated them. It would be interesting to supply the want of numerical statements relative to the diffusion of encephalosis and of scirrhus in the different organs.

One fact is very remarkable; the encephaloid matter has frequently been detected in the veins, especially in the liver and kidney. It was discovered, accordingly, by M. Berard, that an injection could be transmitted by the arteries, but not by the veins. In a case of cancer of the thyroid gland, it is said—"not a vein was injected, whilst the arterial injection penetrated every part of the tumour." M. Cruveilhier concludes that encephalosis has its seat in the "capillary veins;" and that these become distended and form the cells in which the cancerous matter is contained.

There appears, then, to be, in this disease, the secretion of a peculiar substance of soft consistency; induration and hypertrophy of the cellular membrane; a disposition to the formation or deposit of the former in the veins; such are the facts we possess upon this subject, until the result of Mr. Kiernan's researches shall appear. The disposition to enlargement, by which injurious pressure is frequently induced,—to thickening of canals, by which obstruction is occasioned,—the disposition to pass into ulceration and to form fungus,—the disposition to become diffused in the animal economy, and to affect the system with a peculiar cachexia; these are the terrific characters of this fearful malady.

In connection with the more ordinary forms of encephalosis and scirrhus, several others must be noticed: to the other textures, melanosis is occasionally added; in some instances the disease assumes the areolar and gelatiniform appearance, from the deposit of a jelly-like substance, termed by Laennec *colloide*, in the meshes of the cellular membrane. This last form of can-

cer is most frequently seen affecting the pylorus and adjacent part of the stomach,—and, next to this, the bones.

The symptoms in encephalosis and scirrhus, are, first, those which arise from injurious pressure upon the contiguous organs; secondly, those which arise from obstruction to canals; and thirdly, the state of cachexia, with straw-coloured complexion, emaciation and debility, &c., which frequently result from the influence of the disease upon the system at large. This interesting subject will be specially treated of hereafter. I will only add, in this place, that the organs compressed are frequently,—

1. *The Cerebrum;*
2. *The Orbit;*
3. *The Olfactory, the Optic, and the other Nerves;*
4. *The Medulla Oblongata, &c.*

when the tumour occupies a locality within the cranium;

1. *The Trachea;*
2. *The Œsophagus;*
3. *The Aorta, or Carotid Artery;*
4. *The Jugular Vein,*

with appropriate symptoms, when the tumour occupies the sterno-clavicular region; and,

1. *The Rectum;*
2. *The Urethra;*
3. *The Vagina;*

when it occupies the pelvis; and that the canal most apt to be obstructed is the alimentary, in various parts of its course, as,

1. *The Œsophagus;*
2. *The Cardia, the Pylorus;*
3. *The Ileum, the Colon;*
4. *The Rectum;*

with symptoms equally appropriate.

In encephalosis we have neither *fever* in the commencement, nor *emaciation* during the course, or even *dropsy* at the close of the disease, necessarily. These events are, however, frequently induced by accidental circumstances, especially the peculiar *locality* of the disease.

Scirrhus occurs later in life generally than encephalosis. It is chiefly remarkable by inducing a pallid, sallow, straw-coloured hue of the complexion, with emaciation, peculiar pains, resembling those of rheumatism. But the principal symptoms of encephalosis and scirrhus are those produced by the *mechanical* pressure upon adjacent organs, or obstruction of canals, which arise out of the peculiar and relative situation.

Encephalosis is more rapid in its course than scirrhus, and it usually destroys the patient by its morbid pressure on some important adjacent organ. I may advert to the arrangement just given. If the tumour be situated within the cranium it may induce the symptoms of *compression* of the brain; it may protrude the eye from the orbit; it may induce loss of the sense of smell, of sight, &c., and symptoms of asphyxia, or disordered respiration, according to its precise situation. If it be situated in the lung, it may induce a slow *asphyxia* by compressing the bronchia and trachea. If it be seated differently it may induce *dysphagia*, by compressing the œsophagus. If, on the other hand, it be fixed over a large artery, it may receive a strong movement at each pulsation of that vessel. Before the real nature of the disease is detected, it may thus be mistaken for trachitis, for stricture of the œsophagus, or for aneurysm of the aorta, or arteria innominata. In any case the course of the blood may be impeded in the jugular veins, and the face and neck may be livid and tumid. A slight degree of these phenomena should lead, indeed, to the suspicion of encephaloid tumour within the thorax, and to a recourse to the stethoscope and percussion for its detection. When the disease occupies the *pelvic region*, and the rectum, the urethra, or the vagina be compressed, and their functions interrupted, we are enabled to clear up the diagnosis by a proper examination. Until this is instituted, the disease is frequently mistaken for stricture of the rectum, or urethra, or disease of the bladder or uterus.

Gentlemen, I regard these *practical* remarks as of more value to you than more lengthened observations upon the mere structure of these tumours, which is, besides, little understood, and will be so until we have the advantage of the results of Mr. Kiernan's researches.

I must in the same view, add a very few remarks upon the effects of scirrhus, when this, according to the second part of the arrangement which I have presented to you, occupies some part of the alimentary canal.

If the scirrhus be seated in the upper part of the œsophagus, there is difficulty in the first act of deglutition. If the disease be seated lower down, or at the *cardia*, the first and some subsequent acts of deglutition are accomplished without difficulty, until the œsophagus becomes filled and distended, and then the further acts of deglutition are impossible, and several events occur; the fluid attempted to be swallowed is forced through the nares, and perhaps an act of vomiting is excited by the contact of this fluid with the fauces; this vomiting merely empties the œsophagus—it is œsophageal. If the scirrhus occupy the lower curvature or body of the stomach, it may act upon the *system* without inducing *local* symptoms. Seated in the *pylorus*, pain occurs after taking food, and is only relieved by acts of vomiting. When the disease is fixed in the intestine, it produces pain,—iliac or colic pain, according to its locality,—with a sense of obstruction, and a sensible tumour. When

the disease affects the rectum it impedes the evacuation of that bowel, and is detected by a proper examination.

These are *practical* observations, of which you will learn the value when you begin to visit patients of your own, and to experience the hopes and fears inseparable from the practice of medicine. I shall never forget the case of a gentleman, in whom an encephaloid tumour formed in the upper lobe of the right lung, and, from its pressure upon the arteria innominata, was mistaken for aneurysm of that artery. I shall never forget the case of another gentleman in whom a similar tumour compressed the trachea and large bronchia, and produced the symptoms of tracheitis and bronchitis, and eventually a slow and terrible asphyxia. In this patient the large veins of the neck and arms were compressed, and the face and hands were tumid with venous blood. You doubtless remember the case of fungous disease of the cardia in a gentleman of Fowey, in Cornwall. Two of our first surgeons were mistaken in his case; one without the bougie, the other with it. He could swallow a large cup of cocoa, but if he attempted to take more, it regurgitated through the nostrils and induced vomiting. This deglutition, and this vomiting were œsophageal; the stomach was put *hors du combat* by the stricture of the cardia. Twenty-five years ago, or nearly, I had a patient who was never easy after taking food, to which he was prompted by an irresistible hunger, until he had provoked an act of vomiting by tickling the fauces. It was worse than the cup of Tantalus. His case was scirrhus of the pylorus. My own first and dearest friend, Dr. Andrew Boutflower, the companion of my Edinburgh studies, an elegant scholar, and a most amiable man, died of scirrhus of the rectum. His sufferings were those of an interrupted intestine, joined to a daily increasing debility and exhaustion.

Whenever and wherever encephalosis occurs externally, always remember that it is a *diffused* disease, before you think of extirpation or amputation. Examine the liver; you may find it already enlarged with tubera. Observe whether the patient be not already affected with pallid lips and emaciation. The same precaution applies to scirrhus. In this latter case, however, I believe that life is prolonged, and suffering diminished by an early recourse to the scalpel. The only other remedy which has appeared to do good, is the perfect quiescence of the part affected; a plaster, worn for years, has staid the progress of scirrhus of the mamma; whereas marriage, in one case, immediately developed a disease of the uterus not suspected before.

I must add a few words upon the subject of cancer. This term, or carcinoma, is applied to many intractable and destructive ulcers, but especially such as result from encephalosis and scirrhus. It is frequently a question, especially in the contemplation of excision, whether a given ulcer be cancerous in this sense or not: when such ulcer is situated in the lip, in the tongue, for instance. Facts are wanting to solve this momentous question.

Besides the diseases which I have just noticed, others will be treated of in the course of the subsequent lectures, in connection with the organs in which they appear principally; they are chiefly the encysted, the fibrous, the polypous tumours. It is only necessary, in this place, to point out the necessity for a strict diagnosis between these tumours and encephalosis and scirrhus.

Gentlemen, brief as this lecture may be, I shall not enter upon new topics this evening; I will merely remind you that tubercle and melanosis contaminate the whole system, and prove fatal, from their influence upon the processes of absorption and nutrition in general. The tuberculous patient, especially, dies from inanition and emaciation. Encephalosis and scirrhus also contaminate the system, but rarely prove fatal in this manner. Tubercle is without *tumour*. The tuberculous lung is little, if at all, larger than the healthy lung. Encephalosis, especially, and generally, also, scirrhus, are, on the contrary, characterised by *tumour*, by morbid growth and enlargement; in this manner they compress organs, or obstruct canals essential to life, and in this manner they generally prove fatal. Encysted and fibrous tumours do not contaminate the system at all, and prove injurious by their bulk, and by the compression or irritation which they occasion. Polypous tumours, especially those within the uterus, frequently induce excessive hæmorrhagy.

In our account of encephalosis we ought ever to mention the names of Hey, and of Abernethy, but especially that of Mr. Wardrop, whose treatise on this subject is invaluable. I have always regretted that Mr. Langstaff did not publish his *cases* as he formed his *museum*. Had he done so, he would have anticipated the illustrious Laennec in his important additions to this part of pathology.

In my next lecture I shall introduce to you the first of a series of subjects which differ from those with which we have been recently engaged, by being more amenable to the influence of remedies. Fortunately, too, they constitute the more usual events in practice. In two of these I have myself taken special interest. They are *intestinal irritation*, and *the effects of loss of blood*.

LECTURES
ON
MATERIA MEDICA AND THERAPEUTICS;
NOW IN COURSE OF DELIVERY
AT THE
WINDMILL-STREET SCHOOL OF MEDICINE
BY
GEORGE G. SIGMOND, M.D.

MERCURY (continued):—*Compounds of mercury with oxygen. The oxide of mercury known to Aristotle; method of preparing this salt. The nitric oxide of mercury. The compounds of mercury and sulphur; mercury and chalk. The medicinal effects of this metal and its salts; their peculiar effects. Action of mercury in the cure of lues venerea considered. Experience of Morgagni, Boerhaave, Van Swieten, and others.* CORROSIVE SUBLIMATE; *when first employed; its effects, mode of action, and dose. Experiments with it, made by Van Swieten. Neglect of this salt by British practitioners.*

GENTLEMEN:—Mercury unites with oxygen in two proportions, and the preparations which are the result have admission into the Pharmacopœia, under the names of hydrargyri oxidum and hydrargyri binoxidum. The first of these is obtained from an ounce of the chloride of mercury and a gallon of lime water: mix, and frequently shake them; set by, and when the oxide has subsided, pour off the liquor; lastly, wash it in distilled water until nothing alkaline can be perceived, and dry it in the air wrapped in bibulous paper. Here the chlorine of the chloride enters into combination with the calcium of the lime, and chloride of calcium is obtained, which remains in solution, and is abstracted in the pouring off of the liquor. The oxygen of the lime entering into union with the mercury forms the precipitated oxide of mercury. Digested for a short time with diluted hydrochloric acid and strained, neither solution of potash nor oxalate of ammonia throws down any precipitate. It is totally insoluble in acetic acid; by heat it is entirely dissipated. It ought to be nearly black; it is nearly insoluble in water, but soluble in the acids, forming a peculiar class of salts.

Mercury in this slightly oxidized state was known to Aristotle, who states that on mixing this metal for a long time with the saliva a useful remedy is obtained in the cure of some of the diseases of the skin; but, in modern times, it was Boerhaave who drew attention to it. He observed, that whether we rub the mercury with the hand, agitate it in the air, or shake it in a bottle, triturate it with any thick viscid liquid, that it is converted into a black oxide, which is again restored to the state of fluid quicksilver by the action of fire, and even of light. This powder, from its colour, he called "Æthiops per se."

The complete oxidation of mercury is the result of a process described by the older alchemists; it was well known to Boyle, and he invented a particular apparatus which was long known under the name of "Boyle's Hell," in which it was said he tormented the metal. This oxide of mercury contains nearly a tenth part of its weight of oxygen; and it was from this that Dr. Priestley first obtained oxygen in its pure state in the year 1744, and upon the investigation of its properties was laid the foundation of the doctrine of the gases now so universally acknowledged to be the great source of the discoveries of modern chemistry. It is the hydrargyri oxydum rubrum of the last Pharmacopœia; and the formula, as at present laid down, is,—Take of bichloride of mercury four ounces, solution of potash twenty-eight fluid ounces, distilled water six pints, dissolve the bichloride in the water, strain, and add the solution of potash; wash the powder thrown down in distilled water, until nothing alkaline can be perceived, and dry it with a gentle heat. On the mixture of the bichloride with the solution of potash, decomposition occurs; the two parts of chlorine which exist in the bichloride take two of potassium; hence two parts of chloride of potassium remain in solution, whilst the two parts of oxygen combine with one part of mercury, and this is precipitated, forming an orange-red powder. If this be heated in open vessels it sublimes, and the result is a transparent vitriform substance of a singularly beautiful ruby-red colour; if heat be applied, it yields oxygen, and the mercury either runs into globules, or is entirely dissipated: it is completely soluble in hydrochloric acid; it is very acid to the taste; has no odour. Boerhaave tried many experiments with the red oxide of mercury; and the greater part of our knowledge, as to its chemical characteristics, are owing to his labours. Neither of these salts have acquired much reputation in the cure of disease; the oxide is given in the form of pill, in the dose of a grain; the binoxide is very active, it excites vomiting and purging; but it ought rather to be considered as a poison than a medicine, for its effects as the latter are very uncertain, whilst its deleterious powers are most decided. Neither of these salts has ever been the subject of any very peculiar inquiry by medical men, with the exception of the learned Boerhaave.

There is another preparation in the Pharmacopœia very rarely employed internally, in which the mercury is in the state of

binoxide, containing also a little nitrate; it is called, "hydrargyri nitrico-oxydum," and was formerly known by the name of red nitrate and red corrosive mercury; it is thus formed,—Take of mercury three pounds, nitric acid a pound and a half, distilled water two pints; mix them together in a proper vessel, and apply a gentle heat until the mercury is dissolved; boil down the liquor, and rub what remains to powder; put this into another very shallow vessel, then apply a slow fire, and gradually increase it until red vapour arise. This preparation is of a bright-red colour; it resembles, in most points, the binoxide. On the application of heat no vapour is evolved; neither lime-water nor hydrosulphuric acid throws down anything from the water in which it has been boiled. The ammonio-chloride of mercury was formerly called the white precipitate, and occasionally used as a remedy externally in cutaneous disease. It is formed of six ounces of bichloride of mercury, distilled water six pints, solution of ammonia eight fluid ounces; dissolve the bichloride of mercury with the application of heat in the water. To this, when it is cold, add the solution of ammonia, frequently stirring; wash the powder thrown down until it is free from taste; lastly, dry it. This preparation is completely evaporated by heat. When digested with acetic acid, iodide of potassium throws down nothing, either yellow or blue. It is totally dissolved by hydrochloric acid, without effervescence. When heated with solution it becomes yellow, and emits ammonia. There are different theories as to the composition of this salt.

Mercury very readily enters into union with sulphur, both by simple trituration and by the action of heat. The first operation performed was the rubbing together of the liquid metal, and two parts of sulphur, which was called, from the black colour that was assumed, the æthiops mineral. A salt, called the hydrargyri sulphuretum cum sulphure, and formerly hydrargyri sulphuretum nigrum, has now a place among our medicines, and is directed to be made of equal parts of sulphur and of quicksilver, which are to be rubbed together until globules no longer appear. It is not only a simple mixture that is the result of this trituration, but the sulphur actually combines and adheres to the mercury, and separation cannot take place but by chemical means. Still, however, a strong magnifying glass enables us to see oblong globules of mercury; this is a very black insipid powder, which diffuses a fœtid smell; it whitens gold when rubbed upon it, and totally evaporates by heat. At one time it was commended as a powerful cutaneous medicine, and in the dose of half a scruple was administered; it has, however, fallen into disuse.

The black sulphuret, by fire, or the æthiops mineral, by fusion, was variously prepared by the alchemists, but most of their processes were susceptible of detonation, and therefore accidents very often occurred in the laboratory. Bayen, indeed, has shown, that the oxides of mercury fulminate by a small quantity of sulphur, when heated together strongly.

The bisulphuret of mercury is the artificial cinnabar of chemical authors; the red sulphuret of mercury of the former Pharmacopœia. Great difference of opinion has existed as to the relative proportions of the principles, and as this salt is in great request in the arts, on account rather of its beautiful colour than of its medicinal virtues, great attention has been paid to its manufactories. In "Crell's Journal," and in the "French Annals of Chemistry," is much curious matter upon its proper fabrication. It is directed to be made of two pounds of mercury, five ounces of sulphur; mix the mercury with the sulphur, melted over the fire, and as soon as the mass swells remove the vessel from the fire, and cover it strongly, lest inflammation should occur; then rub to powder, and sublime. In the first part of the process combination takes place between the mercury and a portion of the sulphur; by continuing the heat, the excess of the sulphur is diminished, and, upon sublimation, this salt is obtained in mass of a dark, or dull-brick colour; but when it is powdered, a beautiful vermillion is produced, which is unalterable by the air; but several of the metals possess the power of separating the mercury. It is used in mercurial fumigations, heat being applied.

A preparation of very great utility is formed by the union of mercury with chalk; by triturating together three ounces of the metal, and five ounces of prepared chalk, until globules are no longer visible, a mild mercurial preparation is obtained, which was formerly known by the name of the alkalised mercury; there are three other formulæ in the last edition of the Pharmacopœia. The bicyanide of mercury to which I have before alluded, composed of two equivalents of cyanogen and one of mercury; the iodide composed of one equivalent of iodine and one of the metal, and the biniodide formed of two equivalents of the iodine and one of mercury; but as these are scarcely known to us as mercurial preparations, I shall defer speaking of them until the properties of iodine become the subject of my consideration.

Besides these preparations there is a vast number of others which have been at various times highly extolled by medical men. We find the milk of mercury, the mercury of life, the mercurial tincture, and a long catalogue of recipes are collected by Swediauer, and other writers who have availed themselves of the knowledge of the

older alchemists. The "Pharmacopœia Syphilitica of Swediauer" contains a methodical table of these remedies.

The chemists, and amongst them Fourcroy, considered mercury only as medicinal, from its being an oxyphorous substance; that is, a substance which possesses the power of imparting to the animal economy oxygen; and they believed that the good effect that was derived in the cure of disease solely depended upon the superior power of the different preparations, according to the quantity of the aerial fluid each contained, and they divided them into four classes. In the first those were enumerated which were in the least state of oxidation, such as the oxide of mercury, which was prepared by various processes, by the addition of mucilage, of syrup, or by the saliva; in the second class were those salts which have but sparing solubility, such as the chloride or calomel, and are but little oxidated; in the third were the oxides of mercury which were in union with sulphur, and whose action is but very slight, and only when they are in the form of vapour; the fourth class was composed of those mercurial remedies which are more highly charged with oxygen, either alone or in combination with other acids; amongst these the nitric oxide and the bichloride were ranked, and to this class the general name of oxygenated caustics was applied. But this theory did not long maintain its ground, and various have been the speculations which have been entertained as to the peculiar mode of operation of the different salts, which, although they all have certain properties which belong to them as a family, differ most materially each from the other in the intensity, the duration, and the celerity of their action.

There are marked characters which peculiarly distinguish them from the salts of all other metals, and the most striking are the effects upon the salivary glands, their power over the syphilitic disease, and likewise their so influencing the human economy as to produce affections which bear a very striking resemblance to the symptoms which the metal has the power of curing. No substance with which we are acquainted has such a diversified operation upon the animal economy; its effects are so various, it influences so many organs, the phenomena it excites are so remarkable, that we cannot feel surprised that so much has been written upon it, and that there exist so many opinions, though not decidedly at variance with each other, which often clash and render a thorough elucidation of all the circumstances connected with it a matter of very great difficulty.

Amongst the subjects of the deepest importance to society are questions that have been agitated by some of the most learned men, who have combined with acute powers of reasoning, great practical information, that have not entirely been decided, and to which I can only cursorily advert. One of these inquiries has been directed to ascertain whether the venereal disease can be cured without the employment of mercury; and another also remains for discussion, which of the preparations of mercury is the most useful.

To the first of these points the answer I would make is, that the cure may be effected, but it demands a thorough knowledge of the stage and state of the disease, the time at which it was first contracted, and the constitution in which it appears; and with regard to the second, nearly the same answer may be returned, but the particular remedy more depends upon the stage of the disease. You will find in a large class of your patients great dissatisfaction exist, if you have not employed to some extent mercury, and should they during two, three, four years, or even for a longer period, have a pimple upon the face, or an ache in the leg, you will hear them declare that they believe that it is owing to their not having been perfectly cured when they laboured under this affliction; and practitioners are often obliged, in compliance with the earnest anxiety of their patients, to have recourse to mercury when they do not feel perfectly satisfied either as to the necessity or the utility of this remedy. Hennen, who in his "Principles of Military Surgery" has a short treatise upon syphilis, which is one of the most valuable little essays upon the subject which is to be found in our language, quotes an old author whom I have never chanced to meet with, Nicholas de Blegny, who quaintly says, "When pocky people have been told that they cannot be recovered but by salivation, and that they are only quacks and empirics who promise to cure it by other means, they become deaf to all further instructions and advice, and firmly believe that all other methods are dangerous and erroneous."

Amongst the reasons that are now advanced for the administration of simpler medicines, such as the acids, sarsaparilla, guaiacum, bark, hydriodate of potash, where mercury would formerly have been given, is an idea that seems generally prevalent that syphilis has, during the lapse of years, changed its character so far as to be of a much milder nature, and much more tractable. Doubtless, during its first appearance, it was often combined with diseases which arose from filth, from bad diet, and from neglect; leprosy and scurvy at that time were the scourges of the people, and the new disease often attacked those whose constitutions had been previously harrassed by these enemies of the human race. We very speedily, however, find the authors congratulating the world upon the milder form in which the disease appeared. Ulrich de

Hutten, in 1519, says that its virulence in Germany had somewhat abated; in 1563 Bernardinus Tomitanus, of Padua, foretells the decay of the disease, and thinks it will soon be no longer communicable by the intercourse of the sexes. He states that when first this malady appeared all the innocent pleasures of society were checked, but that they began to be again indulged in, and convivial parties once more assembled; and he shrewdly observes, that once again marriages were contracted, and that parties began to be more inquisitive about the settlements that could be made than about the disease they had lately so much feared. Astruc collected the testimonies of various authors who believed that the disease was milder, and adds his own; he says, "I have, by careful and repeated observation, found the venereal disease to grow milder; it may, perhaps, be more frequently contracted than formerly, yet its rage is less violent, its symptoms are not so many, so painful, nor so difficult to be cured, it yields more easily to remedies properly applied, and, in a word, seems by little and little to approach towards its close." This was in the year 1735, just a century ago.

Of the frightful ravages committed by the disease, we may best judge from some extracts made by Hennen, from the rare volume of Ruy Dias de Isla, "Contra las Bubas," published at Salamanca, in 1550, from which it appears that in no town in Europe, of one hundred inhabitants, were there fewer deaths than ten, on the first appearance of syphilis; he adds, now if we recollect that the numerous monks and nuns spread the disease through all classes of society, it is not an extravagant calculation, that fifty out of the hundred were diseased; but, putting the mortality out of the question, Ulrich de Hutten expressly states, that hardly one in a hundred was perfectly cured, the disease returning on them again, as it did on himself, after eleven salivations.

Various were the attempts made throughout Europe, to prove that mercury was not necessary in the cure; guaiacum became the fashion,—it was sold at an enormous price,—a medicine which nearly corresponds with our compound decoction of sarsaparilla, and which was called the decoction of the woods, was the celebrated remedy.

Morgagni, whose veracity is considered unimpeachable, tells us that when, as a young man, he went to Bologna, that both the external and internal methods of using mercury were so far abandoned, that he never saw any physician make use of it, or even heard of his using it for the whole space of eight years, during which he studied physic there; he then says, "What remedies, then, have you seen those very excellent physicians make use of, you will say, against the lues venerea; why, the decoction of the woods;" nay, it was soon the fashion to ascribe to the mercury many of the ravages which had previously been laid at the door of the disease. Fallopius, Fernelius, and Palmarius speak of the bones becoming affected by its use.

Since that time various have been the means which medical men have had recourse to in the treatment of the malady; and, from John Pearson's observations on the effects of various articles in the cure of lues venerea, we may draw the inference that almost every article known to us in the materia medica, as possessing any real influence upon the human economy, has, on one occasion or another, been tried; indeed, we are taught from the description of Africa, given to us by Leo Africanus, that the disease is there spontaneously cured by a change of climate; this has been confirmed by a recent traveller, Horneman, who says there are various sorts of venereal disorders prevalent in Fezzan; that imported from Soudan is the worst. The common lues venerea, brought from Tripoli, or Caer, is called the frank evil, or franzi; for the cure of either species they use salts and the fruit candal colocynth, as powerful cathartics, and the sores, if any, are at the same time washed with natron water, or dissolved soda. These remedies seldom fail, unless the disease has taken a deep root.

The aphorisms of that great professor, Boerhaave, which have been commented upon so admirably by Van Swieten, were received as the law of medical men throughout Europe, for a long space of time. His aphorism 1458, inculcates that gonorrhœa is cured by the bath, by fomentation, by injection, by mercurial purgatives, often repeated, by emulsions, by balsams, by abstinence from all food or drink which can stimulate, and attention to moderate diet; that numbered 1467, teaches us that where pustules, pains of the limbs, large buboes, agony in the bones, nocturnal distress are present, a mercurial salivation is demanded. Upon these two aphorisms Van Swieten has bestowed great labour and learning, and terminates with his expression of his dislike to the external application of mercury, and to the commendation of the use of the bichloride, or corrosive sublimate.

Boerhaave recommended that every two hours a dose of mercurius dulcis, which is very similar to the calomel of the present day, should be taken until three or four pounds of saliva were daily excreted, and this was to be continued until the symptoms all disappeared, and the time estimated was commonly six-and-thirty days, after which a gentle action upon the salivary glands was kept up for the same number of days; and the concluding aphorism is, no other remedy is requisite for the restoration to health. Such was the discipline for a long time pursued. In this country the same

plan was followed, but rubbing in the ointment was preferred; and we learn that such extravagant loads of ointment were employed, and with such irregularity, that the most deplorable consequences were too often witnessed.

About the year 1757, the corrosive sublimate was introduced into this country, under the auspices of Sir John Pringle, as the best salt to be used in the cure of the disease. In the first volume of the "Medical Observations and Inquiries," will be found a very interesting paper, entitled, "The Cure of the Lues Venerea by the Mercurius Corrosivus Sublimatus," communicated to a member of the *Medical Society*, by Abraham Gordon, surgeon-major to the 3rd regiment of Foot, commanded by Colonel Howard. It gives the narrative of the method of administering a solution of this salt, and of the success it had in the regiment, then encamped in Dorsetshire, where there were a great number of venereal cases, with little accommodation for salivating. It consisted in giving the corrosive sublimate dissolved in malt liquor, or in French brandy, or the common molasses-spirits of the country, in the proportion of one grain of the mercury to two ounces of the spirit; the dose was from a common spoonful, or half an ounce, to two spoonfuls, or an ounce, twice in the day, adjusting the quantity to the strength of the patient, and the virulence of the disease. The solution was continued as long as the symptoms remained, and the patient was restricted to a low or spare diet; the drink, plenty of barley-water, with a little milk, or any diluting liquor. Twenty cases are related, in all of which the treatment was eminently successful. The action seems to have been confined to the skin and to the kidneys, for both the perspiration and the urinary excretion were increased. Mr. Boyd, surgeon to another regiment, was equally successful. The author says, that from what he has seen himself, and learned from its success in the other field hospitals, he greatly prefers this medicine to a salivation, or any other mercurial course he has tried, during the fifteen years of his service in the army, whether he considers the certainty and the speediness of the effects, or the ease and safety of the cure.

It was to the illustrious Baron Van Swieten we were indebted for the original introduction of this system of cure, which unfortunately has been again abandoned. It was at the *Military Hospital* at Vienna, that Van Swieten first made a series of the most important trials, for his attention had been drawn to the dreadful evils inflicted by the long and cruel system of salivation, which at that time pervaded Europe. Such, however, was the outcry raised against this innovation, that there was the utmost difficulty in obtaining a fair trial; the plan was called impious, cruel, inhuman. Probably one of the most interesting dissertations to be found in medical literature, is the one in which this skilful physician gives us the narrative of his labours, and his views of the treatment by salivation. It is contained in the elegantly written commentary upon the 1477th aphorism of Boerhaave.

The incredible horrors to which an unfortunate young man was in those days submitted, for the cure of the venereal disease, are related with such apparent fidelity, and such painful accuracy, that those who read them must feel grateful that they are born in an age when the light of truth has dispelled the fearful gloom in which mankind walked, and has made the science of medicine the source of happiness and of comfort, instead of a scourge, a torture, and a curse.

The tender infant was submitted to the same treament, for we learn from this dissertation that whole families were diseased, and had to undergo the same miserable course, until a long protracted salivation occurred. This excellent man saw that this was unnecessary, and he gradually was brought to the conclusion that the bichloride, or common corrosive sublimate of former days, would be a more active remedy; it is not possible to do justice to the reasoning upon which he founded his system by any outline, I must recommend to you its perusal.

It was in the hospital called *St. Mark*, which had been founded by some benevolent people in the immediate neighbourhood of Vienna, for the cure of persons affected with the venereal, that Van Swieten first commenced his mode of treatment. In this institution, from its foundation, there had been some recipe employed which was kept a secret; it was, however, a mercurial, and the administrator was not required to be a medical man; this excellent man, however, obtained an order that, in future, the hospital should be under the direction of proper skill, and Maximilian Locher was appointed, who found, in May, 1754, a hundred and twenty-eight patients confined to bed, awaiting salivation; upon these the corrosive sublimate, in spirit, was tried, and they were all cured. In eight years there left this institution thus cured, no less than four thousand eight hundred and eighty persons; this plan was persevered in until the death of Locher, who declared that no one had ever died in consequence of the remedy, and that no dangerous or severe symptom supervened; unless there was on the *primæ viæ* anything to be removed, no other remedy was found necessary to be administered. Men were more easily cured by this mode of exhibition of mercury than females.

Three hundred soldiers were sent to *St. Mark*, and Locher wished to reject six as totally incurable; they had often suffered from the disease, they had almost all their

bones carious, they entreated, with prayers and tears, to be allowed to undergo the treatment, and, accordingly, with the others, they were submitted to the cure by corrosive sublimate, but, upon them, no good effect was produced, whilst all the others left the establishment perfectly cured; but what was the astonishment of Van Swieten when a document was placed in his hands from the Council of War, stating that the physician had not only dismissed these three hundred men, without effecting a cure, but that they were actually worse than before. The records of the hospital, however, showed the names of the soldiers, the companies, the regiments to which they belonged, the day of entrance and the day of departure of each. By good fortune all of them happened to be, at that time, stationed in Vienna; the physician, of course, demanded an inquiry. Under various pretexts this was delayed, and, in the mean time, the calumniator disappeared, and Van Swieten triumphantly quotes the language used by Cicero when Catiline left Rome: "Abiit, excessit, evasit, erupit." He says, those who do not like this plan are quite at liberty to try any other, and certain it is that patients who are afraid of it will find plenty of practitioners who will have no objection to try the old system of torture by salivation. This plan soon afterwards gained ground in all the military establishments in Europe, and officers were generally pleased to find the soldiers not only quickly restored to their duty, but, upon fine, open, cold, and sunny weather, capable of going through much fatigue without any complaint.

Hennen observes, that, independent of the details in books, innumerable opportunities have been afforded to British surgeons, since the peace, of seeing the unquestionable efficacy of this medicine at the hospitals in Paris, and yet a very few years since no orthodox English practitioner would have trusted his patients' safety to it. I believe that, in the first stage of the venereal disease, no remedy more quickly, safely, and certainly acts than does corrosive sublimate thus administered, and I am somewhat surprised that it is not more universally employed by medical men. In the later stages it is, however, not to be depended upon, for in secondary syphilis it only suspends the action of the virus, which again displays itself; it always demands the greatest caution and determination. It is the most common mode followed by the uneducated classes, and the quack very often gains a good livelihood by his knowledge of the value of this salt. I believe it enters into the composition of "the vegetable syrup." It has been, on most occasions of this sort, given in combination with sarsaparilla, or sarza, as it is now to be called, and also with the decoctions of the Peruvian and other barks, upon all of which it exerts some chemical changes, but with which, nevertheless, it is very usefully employed, and also in cutaneous diseases of long standing, and in scrofulous sores. Willan thinks it the only useful preparation of mercury in lepra, its operation being promoted by the use of an antimonial, or the decoction of wood. It is likewise much commended as an external application; a grain in six or eight ounces of rose-water, has obtained a reputation in venereal ophthalmia. It is decomposed by a great number of substances which renders it rather difficult to be administered in composition, and ordered in extemporaneous prescription. Thus, it should not be given with almond emulsion, with infusion of camomile, with nitrate of silver, with acetate of lead, with tartrate of antimony. It is partially decomposed by the action of light, and, therefore, should always be kept in opaque bottles.

CLINICAL LECTURES AND REMARKS ON DISEASES OF THE SKIN, VENEREAL DISEASES, AND *SURGICAL CASES*,

DELIVERED AT THE

SKIN INFIRMARY, AND AT THE JERVIS-STREET HOSPITAL, DUBLIN.

By Wm. WALLACE, M.D., M.R.I.A.,

Honorary Fellow of the Medical Society of New York;

SURGEON TO THE ABOVE INSTITUTIONS.

REMARKS ON THE NEGRO'S SKIN.

(*Concluded.*)

References to the last lecture. Colour of cicatrixes in Negroes. Errors of Bichat and Meckel relative to this subject. Are the epidermoid tissues inorganic and dead, or organised and living? The opinion that the epidermoid tissues are produced by desication of the outer layers of the cutis considered. The opinions that the rete is produced by an effusion from the cutis, and that the cuticle is desicated rete, considered. Varieties presented by the cuticle in different parts. Cuticle of the fœtus in utero. Structure of the epidermoid tissues in the whale. Conclusion. The influence of the chloride of lime on the colouring matter of the Negro's skin, when on, and when removed from the body; inferences. Phenomena sometimes presented during the regeneration of the epidermoid tissues. Structure of the cuticle as unfolded by maceration. Sensibility of the denuded rete. Influence of the epidermoid tissues on the functions of the cutis. Diseases of the

epidermoid tissues. Conclusions. Remarks on the opinions of Gaultier and Breschet. A reconciliation of those of Gaultier with the facts observed.

GENTLEMEN:—The two last lectures were occupied with the consideration of the effects of blisters applied to the negro's skin, and of the consequent actions of reparation. The facts elicited I employed to test the validity of some opinions and assertions of the French authors, Breschet and Gaultier, and to illustrate some, peculiar to myself, and opposed to theirs. The subject is, however, still far from being exhausted; but if I were to enter into it, in all its bearings, I should occupy time which must be devoted to other objects. I shall not, therefore, detain you longer than to say a few words on certain topics which are of too much importance to be passed in silence. You remember there were

Cicatrixes on the Cutaneous Surface of the Negro;

some caused by wounds and some by ulcers. For example; he had lost the extremities of several of his fingers by frost-bite; he had received sabre-wounds, and some smaller wounds, on his right arm and eyebrow; and he had had several ulcers on one of his legs. Now, what remarks had we occasion to make respecting the colour of these cicatrixes. We observed, that some of them were black, and that others were partly black and partly white,—black at their circumference and white in their centre. The cicatrixes of the wounds on his arm and eyebrow were black. Those which followed the smaller ulcers on his leg were also black, while those which the loss of the ends of his fingers, and the larger ulcers on his legs had produced, were black at their circumference and white in their centre. From these facts, as I observed to you, we are authorised to conclude that Bichat, and those writers who affirm that cicatrixes in negroes are uniformly white, as well as Meckel, and those who affirm that they are always black, are equally mistaken. In fact, cicatrixes, when fully formed in the negro, may be either white or black. They are black when the previous solutions of continuity have been superficial, or of small extent, but white when there has been much loss of substance. I say, such is the case when the cicatrixes are fully formed; for you are to remember, as I have already pointed out, that all cicatrixes in the negro, following injuries which have caused the removal of the rete, are necessarily white in the first instance, although many of them subsequently become black. This transient white colour is, no doubt, the reason why some persons have concluded that cicatrixes in negroes are always white.

We had reason to remark, that although the white cicatrixes in Nicholls were without the pigment, which gives the dark colour to the skin, they were provided with cuticle. Hence we must conclude, that parts are capable of forming cuticle which are apparently unable to produce, even after a time, the colouring matter. This fact would seem to argue, that the apparatus for the production of the colouring matter is distinct from that which produces horny matter, or cuticle. But, upon deeper reflection, such a supposition will not appear necessary. Where bone is usually formed, cartilage, or fibro-cartilage, is occasionally produced, as sometimes happens in the reunion of fractures. This occurrence does not, however, lead us to conclude, that the apparatus for the deposition of bone is different from that which forms the fibro-cartilage which now occupies its place. We arrive at the simple conclusion, that the ossific process on these occasions, when bone is supplied by fibro-cartilage, is imperfect. Now, I presume we may apply the same reasoning, and the same mode of explanation, to such cicatrixes in a negro as are without colouring matter; that is, we may suppose that a white cicatrix in a negro is the result of a less perfect process than that which produces a black one. Again: as white cicatrixes covered with cuticle may be formed on parts from which the entire thickness of the skin has been removed, and with it the apparatus, if any existed, by which horny matter is formed, we are led to the belief, that if there is any such special apparatus it is capable of being regenerated. I shall now discuss another question,—

Are the Parts which cover the Cutis dead and inorganic?

or are they organised and endowed with the properties of life? Let us consider this question; that is, let us apply in its solution, as far as we can, the facts we have witnessed. It is a question of great importance in relation to both the physiology and the pathology of the skin.

I will first consider how far the popular opinion, that the epidermoid tissues are inorganic, corresponds to the phenomena we have witnessed; and subsequently examine, whether there be any, or what, proofs of the organisation and vitality of these textures.

It is affirmed, by many of those who maintain the dead and inorganic nature of the epidermoid tissues, that they are formed by the exterior layer of the true skin, desicated or acted on by the atmosphere. You are, however, acquainted with facts which refute this opinion. You know that the epidermoid tissues in the negro possess a peculiar structure, although his cutis does not differ sensibly from that of the European. But it is evident, that if the epidermoid struc-

tures were formed by a desication of the cutis, they should be the same in the black as in the white races, as the cutis is apparently the same in both.

It is the opinion of a still greater number of authors, that the rete is an exhalation from the surface of the cutis, and that the cuticle is afterwards formed by the desication of the rete. You can, by the facts you have witnessed, refute this hypothesis, although sanctioned by such men as Haller and Blainville.

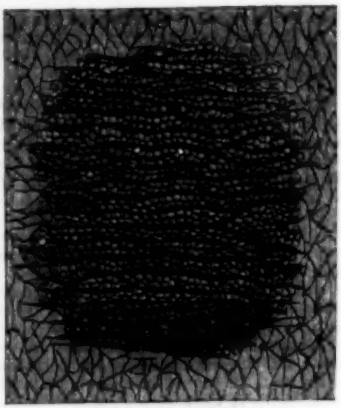
You have seen that after the rete was removed by the application of a blister, a perfect white cuticle was formed before the new rete was developed, at least before it presented a tinge of black. It seems, therefore, more correct to say, that the cuticle forms the rete, than that the rete forms the cuticle.

If the cuticle was produced by the desication of the rete, there would always exist a fixed relation between them; but is this the case? Are

Local Peculiarities

of the one accompanied by corresponding peculiarities in the other? You seemed to take much interest in examining the manner in which different parts of the negro's skin were adapted to the motions of the parts it covers. These two sketches represent the structure of the cuticle which covered the elbows. This was taken when the limb was extended

and this when flexed, and consequently when the cuticle was on the stretch:



Again; here is a drawing of the cuticle of the instep. See how little it resembles

that which covered the elbow. Compare the palmar and dorsal aspect of the thumb, as represented in the following drawing. How different!

Now, those who affirm that the cuticle is the product of the desicated rete, and that the rete itself is an inorganic effusion on the surface of the cutis, satisfy themselves that they have accounted for all such appearances as those to which I have alluded, by saying that they are either the result of the epidermoid tissues being moulded on corresponding forms of the subjacent cutis, or that they are produced by the motions of the part. But such assertions are not reconcileable to the facts, that the cuticle possesses its peculiar conformation while the fœtus is yet in utero; and although, on some parts, as on the hands and feet, the cutis presents a form corresponding to that of the superincumbent cuticle, there are other situations, for example, over the point of the elbow, where, although the appearance of

the cuticle is very remarkable, no trace of a corresponding form could be observed by us in the subjacent cutis.

In all parts of the negro the rete is black, but in none has the cuticle a deeper tinge than what may be called grey or brown, and in some parts it is almost white; nor is there any connection whatever between the relative thickness of those membranes. Compare the rete on the heel with the enormously thick cuticle covering the same part.

The difference between the structure of the cuticle and rete is very remarkable in some animals, but in none more than in the whale.

The Epidermis and Rete in the Whale

have apparently a fibrous structure, but the fibres of the one are at right angles with those of the other. The fibres of the rete are perpendicular, and those of the epidermis parallel to the surface of the animal. In Breschet's work a little drawing, which you here see, is given to represent this

structure. He was, therefore, acquainted with it, and it is amusing to contemplate the devices employed to make this obvious fact tally with those appearances which he says the microscope has unfolded to him.

May we not, therefore, Gentlemen, conclude that the prevailing opinions respecting the mode of production, and, consequently, the absence of organisation and life of the epidermoid tissues are not reconcileable with the facts we have observed. Let us next examine whether there is not abundant reason to conclude that these textures possess a structure and property analogous to the other textures of the body, and hence that they are, like them, organised and living.

One of the best tests of vitality (on some occasions at least) is a power of controlling, to a certain extent, chemical agencies; yet, so far as I know, this test has not been employed by any person previous to myself, to determine the question of the vitality of the epidermoid tissues.

Let me recall to your memory some

Experiments with the Chloride of Lime on the Negro's Skin

made with this object. You have seen me immerse, in Scanlan's solution of the chloride of lime, a flake of the epidermoid structure, removed by a blister from Nicholls, and you observed that its deep black colour was almost immediately discharged. Here is the bottle in which the experiment was performed. You see a mass of white matter in its bottom; this is epidermoid tissue, and it was black when first immersed in the solution; but, you observe, that it is now quite white.

Having thus demonstrated to you that a saturated solution of the chloride of lime has the power of discharging the black colour of the epidermoid tissues of the negro, when these tissues are separated from the body I pointed out to you the effects of the same solution when applied to the living skin.

My mode of applying the solution was, as you no doubt remember, by immersing several folds of lint in it, laying these folds, thus wet, on the surface of the skin, covering them with oiled silk, and applying a roller loosely over all. We examined the surface at the expiration of an hour. What did we observe? The contrary, you will admit, to what you expected. The blackness of the surface was deepened, instead of being diminished; the skin was also rendered tumid, and its sensibility increased; in short, precisely the same effects were apparently produced, which result previous to vesication from the application of a blister.

I now again immersed some folds of lint in the solution, and applied them to the same part. At the end of some hours I made a second examination, and then found the surface as black as before, the cuticle wrinkled, and easily detached from the subjacent rete, the adhesions between these membranes being evidently very much diminished; no vesication, however, had

taken place, and, in this respect, the appearance of the surface formed a striking contrast with that which would have been presented by one to which a blister had been applied.

(Need I remind you that the grey detached cuticle was rendered white immediately on immersion in the chloride of lime, although as long as connected with the living surface it retained its natural colour.)

I next denuded a portion of rete of its protecting cuticle, and applied to its surface the lint dipped, and thoroughly soaked, in the solution of the chloride of lime. The application was now allowed to remain on for some hours, and when removed the coherence of the rete to the surface of the cutis was so loosened that the former could be easily scraped off with the spatula; still, however, it retained its natural black colour, which colour was, as we observed in respect to the cuticle, destroyed immediately on immersing the membrane in the solution.

Nor were these the only experiments of the kind you saw performed with analogous results. Some of the solution was given to the negro, and he was desired to wash his hands frequently with it. This experiment was suggested by yourselves; you saw the consequences: a whitish-mealy appearance of surface was produced on such parts as were covered with thick and rough cuticle. This white appearance could, however, be washed off with water; but, wherever the cuticle was thin, as on the wrists and between the fingers, the black colour was deepened, and the part rendered tumid and tender. Some spots were also stripped of the epidermoid tissues.

Thus it is, that while the epidermoid coverings, when separated from the cutis, are quickly deprived of colour by the action of a saturated solution of the chloride of lime, they resist the action of the same solution so long as they are in vital contact with the body. Can this phenomena be explained unless by allowing to these tissues the properties of vitality? I am of opinion it cannot.

You may occasionally observe, during the regeneration of such epidermoid tissues as had been destroyed by burns or blisters, appearances which seem to argue that

The Rete and Cuticle are formed after the same manner as all Organised Membranes.

I lately demonstrated this to you on Nicholls. A blister had been applied to his leg, and a bulla had formed. Having removed the cuticle of the bulla I dressed the denuded surface with water-dressing, and then allowed the part to remain undisturbed for two days. At the end of this period, on removing the dressing, the blistered surface was found covered with a layer of whitish lymph; this I raised, as gently as possible, but was unable to separate it from the surface on which it lay, without causing the effusion of blood. The lymph was evidently connected to the cutis by vessels which carried red blood; this was not, however, all you saw; the lymph was itself vascular; it, at least, contained in its structure a number of red points; it had, in fact, that appearance which indicates, in new structures, the beginning formation of capillary vessels. Thus the epidermoid tissues are regenerated precisely in the same manner in which the medium of connection between divided parts is formed; that is, an organisable substance is effused, which becomes connected by vessels with the living solids. Now, as, after a time, vessels so contract in new-formed parts, that we find it difficult, sometimes impossible, to demonstrate their existence by injection, although, during the formation of the part, there could be no doubt of it, we never conclude that such structures do not contain vessels; therefore we should not infer that the epidermoid tissues are not vascular, because we are unable to demonstrate their vessels by injection. Moreover, I have, on more occasions than one, seen transparent fibres in the rete, which I presume were vessels.

The Sensibility of the Denuded Rete

to the gentlest impressions, whether chemical or mechanical, several times attracted our attention. A touch with the nitrate of silver was instantly felt, and this even where the rete was so thick that it must, for a measureable time, have protected the subjacent cutis from the action of the caustic. Look at this preparation; it is a piece of cuticle, and you observe even with the naked eye that it is beautifully cellular. This

Cellular Structure of the Cuticle

is still more obvious when you look at the preparation with a magnifying glass. Now, I will tell you how I made this preparation: a man died, labouring under mercurial eczema; when examining his body after death I was able to peel the cuticle off the soles of each of his feet in one flake, like a sandal. Here is one of these flakes, the other I immersed in water daily, as long as any matter separated, and a large quantity of a whitish, pulpy, and granular substance was thus detached. When the cuticle was, after some weeks, cleared of all this matter, I dried it, and hence the preparation which you see It is a valuable preparation for several reasons. In the first place, and this is the reason why I introduce it to your notice, it demonstrates that the cuticle possesses that arrangement of its particles which is most common to organic structures, and, therefore, a proof of its organisation. In the second place, it refutes the opinion of Breschet, that the human cuticle is formed like

that of the whale, of two orders of fibres, one placed perpendicular, and the other parallel to the surface of the cutis; and, in the third place, it removes all doubt as to the question whether the cuticle is, as many have supposed, scaly or laminated.

Unless it be admitted that the epidermoid tissues possess a peculiar and determined structure, and consequent peculiar and determined properties (and are not these proofs of organisation and life?), how can we explain the source of their modifying influence on the functions of cutaneous excretion, absorption, and sensation; or, how does it come to pass that they have the power of remaining adherent, while in a normal state, to those tissues which are admitted to be possessed of vitality? Do we see any other example of a similar cohesion between dead and living matter?

Do not the considerations which I have brought under review abundantly demonstrate the organisation and vitality of the epidermoid tissues? but there are still other facts derived from a contemplation of the phenomena presented by these tissues in disease which add much weight to the demonstration. You appear to inquire what

Diseases Demonstrate the Vitality of the Parts exterior to the Cutis?

I answer, the epidermoid glands being placed outside the cutis, imbedded in the rete, and more closely connected with the cuticle than with the true skin, necessarily constitute one of the divisions of the epidermoid tissues. Now, these glands are the seat of numerous diseases; thus all the papular eruptions, as strophulus and lichen, and many of the vesicular, as miliaria, have their primitive seat in these bodies; of this I have no doubt.

Having long and accurately watched the phenomena of urticaria and prurigo I am of opinion that these diseases are seated principally in the epidermoid tissues, and that the papulæ or wheals that constitute them are owing to an injected state of these tissues; this injected state is often very temporary. Thus the papulæ of prurigo and the wheals of urticaria may be observed this hour, and not the next. The state of the epidermoid tissues which exists in these diseases may be called a state of erection, and it is remarkable that this state is most generally produced by an injection of colourless fluids; and hence the eminences which characterise these diseases, whether papulæ or wheals, are often of a white colour.

The scaly diseases, and many of the maculæ of the classification of Plenk and Willan, are also seated in the epidermoid tissues. We may add a tribe of fungous diseases which have no place in the classification of these authors. That disease which I have described in the "Med. Chir. Trans." by the name of morulus, from the resemblance of the fungus which characterises it to a mulberry, is of this class.

Now, it is remarkable, and important to bear in recollection, that all the cutaneous affections to which I have just alluded, and many others which also have their seat in the epidermoid tissues, however severe their aspect may be, (and sometimes they are so severe in appearance as to render the sufferer a hideous object,) so long as they occupy their especial seat, never leave after them any cicatrix. The surfaces upon which they were seated present, when they are cured, no traces of their former existence. This is an important fact. Often have I observed the ignorant eye astonished at the results of the treatment of such cases. you have an explanation of the occurrence when you consider that although the tissue, the seat of these diseases, may be destroyed, it is capable of being regenerated.

To the facts derived from pathology,—already stated in favour of the opinion, that the epidermoid tissues are vital and organised,—I will add two others.

1st. In consequence of disease, the rete mucosum can sometimes be injected, and then separated in form of a membrane from the subjacent cutis. This membrane was first demonstrated in skin affected with small-pox, and has been named "Baynham's membrane," from the original observer.

2nd. In certain varieties of cutaneous nævi, having first removed the cuticle, we are able to separate a vascular structure from the surface of the cutis without injury to its texture. This vascular structure occupied, therefore, the seat of the rete, and must be considered as an anormal state of this membrane.

Is there not, therefore, every reason to infer, that the epidermoid tissues are living and organised? and of this truth you should never lose sight during your investigations of cutaneous diseases, a class of maladies which yield in importance to none others, whether we consider them on their own account, or, as often revealing to the eye of the scientific pathologist, the existence of deeply-seated and concealed disorder.

Gentlemen: When we commenced the consideration of the subject now concluded, I mentioned that it was more easy to reconcile the representations of Gaultier than those of Breschet, to the facts we have witnessed. If you reflect, you will now observe the truth of this assertion. I have, by the assistance of blisters, dissected, as it were, before you, the skin of the negro. I have demonstrated thereby a cuticle, a rete mucosum, an immense number of granuliform bodies, which I call the epidermoid glands; and lastly, the surface of the cutis. Now, Gaultier affirms, that there are five layers in the cutaneous organs. Let us see how we can reconcile this affirmation with what we

have seen. Passing from without inwards, he describes—1st. A "Couche albide superficiale." This, I presume, corresponds with all that part of the epidermoid tissue which is placed exterior to the epidermoid glands. 2nd. "Les Gemmules." These are, I also presume, the epidermoid glands, which form almost a membrane, or continuous stratum, so closely are they placed to each other. 3d. A "Couche albide profonde." This appears to be so much of the epidermoid tissue, or of the rete, as is placed under the epidermoid glands. 4th. "Les bourgeons sanguines." These are, most probably, the eminences occasionally formed by the points of the cutis, which correspond to the epidermoid glands, or upon which these glands are placed. And, 5thly, the "derme," which is the cutis. Thus, you observe, that so far there is no irreconcileable difference between our observations and the descriptions of Gaultier. Not so, however, when we look at the plates which represent his views. We there observe that he delineates the "Bourgeons sanguines," *bifid*. But if the eminences which he denominates "Bourgeons sanguines" are bifid, they cannot be seated on the part of the cutis upon which the epidermoid glands are placed, for the papilliform eminences sometimes developed on these points are never, so far as I have observed, bifid.

How is the difference between Gaultier's plates, and what we have observed, to be explained? Thus, it appears to me, that he has taken his figures of the "Bourgeons sanguines," from the papillæ on the hands, which, in this situation, pass up between the epidermoid glands, and are, for the reasons already mentioned, bifid; while he has taken his representations of the "gemmules" from the general surface. But, it may be asked, why did he do so? I answer, because the largest gemmules are on the general surface, and because of the size of the papilliform eminences on the hands and feet, which eminences he mistook for those occasionally developed under the epidermoid glands. He has hence delineated epidermoid glands on villi, and has thereby fallen into a striking error.

I shall not attempt so vain a task as to reconcile the account of the structure of the skin which Breschet has given, with the facts we have seen. It would be impossible, yet I cannot help observing, that were I to draw an ideal representation of the cutaneous organ from my knowledge of its functions, I should be very likely to produce a picture far from unlike that which has been furnished to us by Breschet and Roussel de Vauzeme.

DESCENT OF INTESTINE THROUGH THE ANUS.

To the Editor of THE LANCET.

SIR:—I beg to send you the following account of a case which recently came under my notice, and which I consider to be very extraordinary in its nature. I am, Sir, your obedient servant,

GEORGE P. MAY, M.D., M.R.C.S.E.

Maldon, Nov. 19, 1837.

On the 14th of September last, I received a message requesting my immediate attendance upon Mrs. S——. On my arrival I was told by the attendants that "her inside had come out;" knowing, however, that she had formerly some disposition to prolapsus ani, I concluded that the accident was merely an eversion of a portion of the rectum. On removing the bed-clothes I was astonished to see three or four folds of intestine protruding from the anus, and lying in the bed. On examination I found the mucous membrane at the termination of the rectum everted to the extent of an inch, forming a ring through which nearly two feet of the small intestine had passed, the peritoneal surface of which exhibited marks of high vascularity. After much difficulty, owing to the obstinacy of the patient, and her determined opposition to my efforts, I succeeded in returning the gut. In about six hours it descended again, to a greater extent than before; I could now distinctly perceive the visceral attachment of the mesentery. The bowel exhibited a dark chocolate colour, precisely similar to that which is so frequently observed in strangulated hernia. I again returned the intestine, and confined it by a bandage and compress applied to the anus. Symptoms of peritoneal inflammation supervened in a few hours, and proceeded to a fatal inflammation on the fifth day after the accident. I much regret to state that I was unable to make a post-mortem examination of the body, but it appears pretty certain that there must have been a rupture through the coats of the colon, or the upper part of the rectum, through which the small intestine passed.

It is proper to observe that the patient had for three years been the subject of hemiplegia, and her intellectual faculties were much impaired, to which circumstance may be attributed her utter carelessness as to her condition, which was discovered by her friends, quite in an accidental manner.

THE LANCET.

London, Saturday, December 9, 1837.

THERE are few persons, save those on whom devolves the noble, the painful, duty of administering to the wants and necessities of the sick-poor, who are in a position to appreciate duly the immense influence which is exercised by the humane, diligent, and skilful surgeon, over the well-being of the unfortunate individuals who are entrusted to his charge, as contrasted with the effects of carelessness, cold-heartedness, or ignorance, of their medical attendant. The patient who can afford to pay handsomely, as the phrase runs, for the services of a physician or a surgeon, is sure to obtain prompt and efficient medical aid, while self-interest, the most potent of all stimuli, compels the attending practitioner to assume at least a semblance of humanity and of interest for the well-being of his patient; in public or hospital practice the relations which exist between the medical man and the individual whose life is placed in his hands, are essentially of a different nature. The destitute inhabitant of an hospital has no gold to give in exchange for services which may be rendered; he offers no prospect of future patronage to the person who may be the means of restoring him to health; his friendless and abandoned condition is little calculated to excite the sympathy of those in whom the kindly feelings of human nature have been blunted by an inordinate desire of worldly advancement, and should he perish through neglect or mismanagement, the history of his sufferings, were it not for the existence of THE LANCET, would be buried with him in the tomb. It is, therefore, the bounden duty of those under whom the direction or administration of hospitals or other public medical charities is, for the time being, placed, to watch, with vigilant eyes, the conduct of all officers whom they may appoint to such institutions; to stimulate the zeal of the lukewarm, reprove the negligent, punish the guilty, and by a firm, uncompromising exercise of the vast powers which they have assumed, ensure efficient aid to the suffering poor who confidingly throw themselves on them for protection.

In all parts of civilised Europe, save in the United Kingdom of Great Britain and Ireland, the duty of providing medical assistance for the sick-poor, is amply fulfilled by the executive Government, in the establishment of public hospitals, supported at the expense of the community.

In Great Britain, subject as it now is to the Poor Law Amendment Act, a semblance of provision is made for the sick-poor, by the appropriation of a sum of money which admits of about a mite per head being annually paid for medical attendance on paupers, medicines included. This *bountiful* expenditure of the public money, is, however, not quite adequate to meet the necessities of a population which is peculiarly exposed to the exciting causes of disease, and to accidents; hence the necessity, in large cities more especially, of calling upon private charity to supply that medical aid to the destitute sick which should have been provided by the executive Government of the country, were that Government impressed with a proper sense of its duty. We freely and cheerfully admit that the voice of suffering humanity has not been raised in vain. The medical institutions of Great Britain which are, in part or exclusively, supported by the contributions of humane individuals, are not inferior in number, or in intrinsic wealth, to any similar establishments in Europe. Indeed, we may affirm, without fear of contradiction, that a much larger amount of money is voluntarily subscribed by charitable persons in England, for the purpose of supplying medical and surgical aid to the poor, than that which is raised by taxation for the support of public or, as they might be termed, state hospitals in any kingdom throughout Europe. What we complain of is, that these vast sums of money do not produce one-half of the good which would

result from their expenditure under a well-directed system of administration; what we deprecate is, the method of appointing medical men to hospitals universally adopted throughout the British empire; what we demand is, some mode of organisation by which a responsibility for plans devised, and for acts committed, shall fall upon some individual or body of individuals. As affairs are now conducted, we look in vain for more than a pretence of responsibility; yet the slightest knowledge of human nature teaches us, that even the most virtuously-disposed men are led astray by the opportunity of exercising unlimited power.

Public opinion can act but inefficiently towards correcting the abuses which result from the mal-administration of our hospitals, because the officers of those establishments, aided by connivance or supineness on the part of the "governors," generally contrive to escape the ordeal of public exposure on occasions when their conduct loudly demands investigation. Such an exposure, however, is the only remedy that we at present possess, and, as independent medical journalists, we have never ceased, nor shall we cease, to bring to light, and comment upon, such abuses, connected with medical affairs, as may come within our cognizance. It were, undoubtedly, a far more easy task to live in peace with all men, to win the favour and approbation of the corrupt, by unmerited adulation and praise; but, fortunately, Nature has denied us an instinct for the pursuit of these occupations.

The lamentable case of punctured wound, which recently occurred at the *University College Hospital*, is one of those which we have felt it to be our duty to submit to the consideration of the profession and the public. It is prolific of questions of vast importance; hence we shall expose it, from time to time, to the public gaze, and in so doing never omit to require from Mr. SAMUEL COOPER, who did not see his patient until eleven hours after he was admitted into the hospital, a justification of his public professional conduct upon that occasion.

Mr. COOPER, we understand, is stung by our remarks; we are glad of it; but his agony, we suspect, will be less severe than was that of his unfortunate patient.

Mr. COOPER, we are further told, complains that he has been treated unjustly by us, because we preferred founding our comments on some parts of the transaction alluded to, upon the statements of an eye-witness, instead of publishing a report taken from the books of the hospital, and elucidated by observations which this unfortunate surgeon had delivered in the form of a clinical lecture. He shall now be gratified to his heart's content. Ah, SAMUEL, from thine own lips will we now convict thee. But first, we beg to remind the profession, and the thinking and humane portion of the public, what are our allegations against this unfortunate surgeon, in relation to his treatment of his still more unfortunate patient. They stand thus:—

1st. That a man, having a dreadfully severe bayonet-wound of the abdomen, was admitted into *University College Hospital*, under the care of Mr. SAMUEL COOPER, one of the surgeons of that establishment, between the hours of eleven and twelve o'clock at night, and that the patient was not seen by the said Mr. SAMUEL COOPER until a few hours before his death.

2nd. That the patient, during the brief space of his existence, manifested symptoms of strangulated intestine, which escaped the recognition of the surgeon in attendance.

3rd. That after the decease of the patient, when a post-mortem examination had been instituted, a strangulated portion of intestine was found lodged in the wound which had been made by the bayonet through the parietes of the abdomen.

4th. That Mr. COOPER was not present at the coroner's inquest which was held on the body of the slaughtered patient, and thus the responsibility of the case, apparently, fell upon another individual.

Keeping these grounds of complaint in

remembrance, we entreat of the profession to read, as attentively as their astonishment and indignation will allow, Mr. SAMUEL COOPER's defence of his own conduct; and we also entreat the students of *University College* to recollect that their Professor of Surgery, the said SAMUEL COOPER, has been guilty of the impertinence and temerity of accusing us, *ex cathedrâ*, of an injustice towards him, in not publishing the report which we now print, and of not making that report the foundation of such comments as we had to offer on the case of the miserable and unfortunate JAMES CHAPLIN. To this charge we might easily answer, first, that we did not see the report until *after* the first articles on the bayonet-wound case had been printed; and, secondly, that we know hospital surgeons too well to place any great reliance on their own accounts of unfortunate cases. We now, however, publish the report of the clinical lecture alluded to, exactly as it has reached us, merely observing, that it was *actually copied from notes which were furnished to our reporter by* Mr. SAMUEL COOPER *himself*.

UNIVERSITY COLLEGE HOSPITAL.

Penetrating Bayonet-wound of the Abdomen.

"In a clinical lecture, delivered early in November, on the case of James Chaplin, Mr. Cooper observed, that the patient, aged twenty-six, was admitted October 11th, under his care. He was a shoemaker, and having received a wound from a bayonet, in a scuffle with a soldier, at half-past nine o'clock, was brought to the hospital at half-past eleven at night, prior to which he had been seen by a surgeon, who applied a compress over the wound. On admission he was in a collapsed state, face pale and anxious, surface cold, pulse small. A *circular elastic swelling* was discovered in the lower part of the abdomen, about two inches above the middle of Poupart's ligament; in the centre of it was a punctured wound, of half an inch in length, and at the bottom of it a *shining membrane, supposed to be intestine*, was seen. There was no discharge of any kind from the wound. The man continued in the same state until one o'clock, when the pulse began to rise, but again sank on the commencement of *severe and constant vomiting of bilious matter, accompanied by tenderness of the epigastrium*. Two clysters were administered; the first was retained, the second came away with some fæcal matter, and, as was stated, a small quantity of blood.

"Oct. 12. The *vomiting* and collapse continue. Twenty minims of the *liquor opii sedativus* were given, and fomentations employed. Death took place at a quarter past eleven, P.M.

"When he (Mr. C.) first saw the patient in the forenoon of the day following that of his admission, he found the external wound, which was very narrow, closed up, and a soft swelling behind it, which, on gentle pressure being made, slowly and gradually returned, as if it consisted, at least in part, of some effused fluid, either intestinal matter or blood, or *air within the bowel*. He *was not* THEN *aware that anything like intestine had been seen, in the first instance, at the bottom of the wound*, and inferred that the swelling chiefly arose from blood, or other fluid, passing through the wound, and getting under the skin. The man had been in a state of collapse, with a pulse hardly perceptible, and a pallid, cadaverous countenance from the first. This condition never ceased, and implied the wound of some important organ, with or without effusion of blood, or other matter, into the cavity of the abdomen. Had the prostration of the system and pale anxious countenance been the result of mental alarm, they would not have lasted so long. Sickness and vomiting are common in wounds of the alimentary canal, and also in cases of extravasation of blood. He was afraid of exhibiting any active purgative, lest there should be a wound of the bowels; hence the *confined state of the bowels*, and the sickness during the short time the man lived, threw but little light on the question, whether any portion of bowel really protruded, and was strangulated in the wound. Under these circumstances, and being convinced that there was effusion either of blood, or intestinal matter, into the abdomen, he did not divide the skin, being unwilling to disturb the wound, or to lessen the chance of the effusion being bounded by the adhesive inflammation, if it happened to be restricted to the vicinity of the wound.

"The body of the patient was examined

thirty hours after death. An incision was made through the parietes of the abdomen, from the umbilicus to each crista ilii, and the flap, including the wounded portion, reflected downwards. The cavity of the abdomen, thus exposed, was found extensively occupied by effused blood, which amounted, perhaps, altogether to three or four pints. The intestines and peritoneum were highly vascular, and lymph was poured out, so as to render the bowels adherent to one another. A portion of small intestine protruded into the wound, and was *so pinched, or strangulated in it, that when air was blown into it for the purpose of ascertaining whether there was a wound in the protruded part, none of it passed the stricture.* The protruded portion was pierced through by a wound about half an inch in length. In the progress of dissection a wound in the rectum, and one in a branch of the internal iliac vein, were detected, from which last vessel the extravasation of blood had proceeded. Mr. Tayloe was of opinion, that only the serous and muscular coats of the rectum were pierced, but some other gentlemen suspected that the mucous coat was also penetrated. If blood were discharged with the clyster, as the nurse affirmed to have been the case, the wound of the rectum would account for it. This patient, then, died principally from the effects of the wound of the intestine, of copious extravasation of blood into the abdomen, and of *the entangled state of the bowel in the wound.* But if this had been relieved death must have resulted from the peritonitis, the inevitable consequence of so great an effusion into the cavity of the peritoneum.

"The case was particularly instructive, because it raised the question, what ought to be done where there was any doubt of a protrusion of the intestine. To this he (Mr. Cooper) would answer, that if the symptoms were such as only *justified a suspicion, it would be advisable to divide the skin, and ascertain the fact.*

"In the first place, if effused intestinal matter formed the tumour under the skin, the making an opening might be useful, as affording an outlet for the effused matter, and thus lessening the chance of its getting into the cavity of the peritoneum.

"Secondly. If the swelling arose from blood, as he believed it did partly on the morning of the 12th, when gentle and continued pressure made it gradually return, the making of an opening, though not called for in relation to this kind of effusion, would not increase the patient's danger.

"Thirdly. But, if the tumour arose from a protrusion of intestine, and strangulation of it, unaccompanied by profuse extravasation, or other fatal mischief, the division of the skin, and the reduction of such piece of bowel, with or without dilating the opening for its easy return, WOULD *then be the proceeding urgently indicated,* and without which *all chance of recovery would be out of the question.* In one case, as there was a copious and diffused extravasation, and the patient's doom was sealed by it, *the defect in the diagnosis, and the omission to relieve the wounded and pinched piece of intestine,* made no difference in the result. But, under other circumstances, the *omission* would have been a very *serious* one, leaving the patient *no chance* of *recovery.* This case affords one consolation, which is, that it is fraught with valuable instruction, such as will enable all of us to undertake the management of such an accident *in future,* with a full determination to leave no important indication unfulfilled, and in the event of doubt about intestinal protrusion, to make an incision on the grounds and for the reasons which have been already explained. The parts (said Mr. Cooper), exactly in the state in which they were found, are here upon the table. You see the *very close stricture on the fold of the bowel,* and that the portion external to the opening in the muscles, is quite empty and flaccid. It proves that the air originally in it must have been pressed back again into the intestinal canal. There was some appearance of extravasation around the puncture beneath the integuments, while close behind it, within the abdomen, lay a good deal of blood. Now, previously to the wound in the muscles being closed up by the effects of inflammation, did any part of this blood escape through the track of the bayonet, and get beneath the skin, whence it was made to return by the pressure employed? Either this, or the return of the contents of the bowel, must have happened, for the swelling on the morning of the 12th subsided almost entirely under very moderate pressure. You may inquire why bleeding was not resorted to. The answer is, that the collapse in which the patient remained, with cold skin, and a

pulse scarcely perceptible, forbade this measure, so essential in all cases of abdominal inflammation, under ordinary circumstances. As the case was further complicated with a wound of the strangulated piece of intestine, if the stricture had been dilated, the injured part of the intestine should have been kept near the external wound, in order that any of its contents which happened to become extravasated, might pass outwards, and not be diffused into the peritoneal cavity. The various circumstances of this complicated case left the man no chance of recovery under any treatment."

There! Was an act of self-condemnation ever more complete? Beyond this confession of Mr. SAMUEL COOPER's ignorance of practical surgery,—beyond this broad and distinct *confession of his own ignorance*, no evidence can be required.

Our readers are now in a condition to decide, from evidence, which even a person of Mr. SAMUEL COOPER's character will not be disposed to dispute, how completely we were justified in bringing against that individual the several charges which we enumerated in recent numbers of this Journal. To the accusation of having left a dangerously wounded man without surgical aid for more than twelve hours, Mr. SAMUEL COOPER does not respond one syllable. He is necessarily silent on the subject of that neglect.

The fact of the intestine having been strangulated, and the inevitable consequence of such strangulation, the presence of well-known symptoms, are abundantly demonstrated by the words of Mr. SAMUEL COOPER himself,—which we have marked in *italics* in order to supersede the necessity of casting, by comments of our own, additional disgrace on so silly and weak-minded a personage.

The absence of Mr. SAMUEL COOPER from the inquest was one mark of good feeling evinced by him throughout this unhappy and memorable affair. He was probably unwilling to look the dead man in the face, and, through respect for this contrition we shall here simply remark, that it was the bounden duty of the coroner to call for such evidence as was most likely to throw light upon the *real* cause of poor JAMES CHAPLIN's death, and not to receive the testimony of an irresponsible person instead of the evidence of the principal.

The only question, then, upon which Mr. SAMUEL COOPER and ourselves are at issue, relates to the quantity of blood effused into the cavity of the peritoneum. Mr. COOPER states it to have amounted to from three to four pints. We, on the other hand, have been informed by persons on whose observation and credibility we place the highest reliance, that the quantity of effused blood was much less. Now, without dwelling on the respective value of *interested* and *disinterested* testimony, we may advert to the improbability of such a copious effusion of blood co-existing with *rapidly-excited* and *violent* inflammation of the peritoneal lining of the intestines. Such an effect is infinitely more consonant with the idea of effusion of fæcal matter, or strangulation of the intestine. The former did not exist in the case of CHAPLIN, and we leave it to our professional readers to determine whether reduction of the calibre of a gut to such a degree as to *prevent even the passage of air*, affords, or not, pretty conclusive evidence of the existence of strangulation.

But the most curious and instructive part of Mr. SAMUEL COOPER's own report, now published by us, is to be found in the concluding paragraphs, in which the treatment and complications of penetrating wounds of the abdomen are laid down with considerable care and precision. Mr. COOPER evidently betook himself, *after* the loss of his patient, to his study, and there the ingenious compiler concocted a "clinical lecture" from his own "Dictionary," in the same way that the "Dictionary" itself was concocted from the writings of the illustrious dead. Would that he had shown the same diligence at the bedside of his patient; but the mind of Mr. SAMUEL COOPER is evidently more adapted for the contemplation of an-

cient histories, and antiquated theories, than for the purposes of practical surgery. Thus accurately are the indications relative to the case of JAMES CHAPLIN, laid down in Mr. SAMUEL COOPER's own report. The various complications of abdominal wounds are carefully described, and the appropriate treatment, under each circumstance, is pointed out; but why did not Mr. SAMUEL COOPER put all this into practice at the bedside of his patient? "The case," says Mr. SAMUEL COOPER, "is fraught with valuable "instruction, such as will enable all of us "to undertake the management of such an "accident *in future*, with a full determina- "tion to leave no important indication un- "fulfilled," &c. This determination is, no doubt, very virtuously formed, but it can afford but a small degree of consolation to the *manes* of the departed patient. We strongly recommend Mr. SAMUEL COOPER to lay aside this *paulo-post-futurum* surgery, and to *fulfil* the indications of each case which may fall under his treatment, *during the patient's life.*

Mr. SAMUEL COOPER, we understand, is excited in a high degree, in consequence of the comments which we have thought it to be our duty to offer on his conduct; and it is said that he has, since the appearance of our strictures, abused THE LANCET in good set terms. We thank him. If he did not *smart* it would be a proof that the lash had not been judiciously applied. As a public culprit, exposure is, in this instance, a portion of the common fate of all persons who are similarly circumstanced with himself. Probably the relatives of the unfortunate JAMES CHAPLIN have feelings as well as Mr. SAMUEL COOPER. Is *their* cause of discontent less urgent,—is *their* agony of mind less poignant, than that of the surgeon who "would" have relieved the strangulated intestine if he had known that such a thing existed. The late JAMES CHAPLIN was admitted into the hospital between 11 and 12 o'clock at night. This vigilant, assiduous, and sagacious surgeon saw him *for the first time* at about *eleven o'clock* on the following day. But there was "collapse." What says the entry in the hospital books? What says Mr. SAMUEL COOPER's *own report*? Why, that at one period the pulse rose, but that, soon after, there was *sickness*, with a *vomiting of bilious matter*. Yes, "there was collapse from first to last," says Mr. SAMUEL COOPER. There were "pints of blood extravasated "into the cavity of the abdomen." Yet, strange to say, the man could raise himself in the bed, and, in a cheerful mood, putting his hand upon the tumour, remark, "I care "not a straw if I get over this." Poor fellow! He felt the pang which the tightly-bound, severely-corded, gut produced. Oh, yes! There was vomiting of bilious matter, according to the entry in the hospital books,—according, also, to Mr. SAMUEL COOPER's own report. Yes, there was a vomiting of bilious matter. Why, whither *could* the bile flow? After having passed from its ducts, where could it run, flowing, as it did, over the excited membrane of an irritated stomach and intestine? What course, we ask, could it take, if it did not escape from the body through the œsophagus? For Mr. SAMUEL COOPER himself tells us, *in his own report*, that even air could not be blown through the strangulated gut,—so tightly were the sides of the intestinal canal bound together by a stricture, which was not discovered until the *ghost* of JAMES CHAPLIN had become the witness of the surgeon's want of sagacity.

The reflections which such a case suggests are too numerous, and too important, to be recorded hastily, or dismissed summarily. Discussions on subjects of this description should be maintained with patience and forbearance, and ought to be resumed, frequently, for the benefit of students and young practitioners. Even Mr. SAMUEL COOPER is profiting by the lessons which we have been, and are, inculcating; and, as we are charitably disposed, although we know that he is not our friend, we shall return good for evil, and render him, before he is dismissed from our service, fully competent to afford relief in instances of strangulated

intestine caused by protrusion and stricture of the gut in punctured wounds of the abdomen.

We hope to be enabled to render Mr. SAMUEL COOPER capable of looking his dead men in the face.

Recherches sur la Valeur Diagnostique des Déformations de la Poitrine, &c.

On the Diagnostic Value of Deformities of the Chest, produced by Diseases of the Thoracic Organs. By E. J. WOILLEZ. 4to. Paris.

IT has often struck us as peculiarly unfortunate that the production of a printed thesis is not, in this country, included amongst the qualifications essential for a medical degree. The only British school at which anything of the kind is required, as far as we are aware, is that of the *Edinburgh University*, and even there the demand is nominal only; for, in fact, from the Senatus not requiring the thesis of candidates to be presented in a printed form, this part of the academical ordeal is given an air of minor importance, and few are willing to exert themselves in order to produce what is, beforehand, judged to be valueless. Why the professors of that school look on inaugural essays as of such trifling consequence we know not; but there seems amongst them a determination to show that such is the case. Else why should the defence of thesis, of which there is such high-sounding mention in the University statutes, be so utterly contemptible a form as it now unquestionably is? Why should the *argumentation* consist, on the one hand, of a few half-intelligible phrases of praise or censure on the part of a professor who, in many instances, gives evident proof of not having read that on which he presumes to decide; and, on the other, of a few random words of reply from the candidate? Why else should we have to record a fact, of which we vouch for the authenticity, that the comments of a learned professor, on one of the essays laid before him last year (1836), were confined to objections to the *hand-writing* of the work,—certain eulogia on calligraphy, and an exceedingly instructive and writing-master-like lecture on up-strokes and down-strokes? But this would have been all very well, had not the gentleman alluded to gone so far, in his enthusiasm for clear and bold hand-writing, as to gravely inform the candidates, that nothing was expected from them but a *plainly-written selection from classical works*. This gentleman was, no doubt, one of the patrons of the glorious stand-still-persons, who will ask you, with the consciousness of sound views twinkling in their eye,—

> Thut nicht ein braver mann genug,
> Die kunst, die man ihm übertrug,
> Gewissenhaft und punktlich auszuüben?*

The consequence of this systematic disregard of thesis is, that an original essay, or one valuable in any respect, is, at Edinburgh, a thing unknown. How different is the case at Paris. There the production and defence of a thesis is, in reality, as it ought to be, the final test of medical proficiency; and in consequence of this, some of the most brilliant works of which that great pathological school can boast, are to be found in embryo in its inaugural dissertations.

We were led to this comparison, which is so unfavourable to our own country, and of which we should, in consequence, be happy to find the justness disproved, by the perusal of M. Woillez's Essay. The work of an ardent pupil of M. Louis,—it is drawn up wholly from personal and extensive observation, and the labour which such a method of arriving at results requires, has been lavishly bestowed. The author first conceived the idea of his undertaking, from seeing the daily examination made by Louis of the form of the chest, especially in cases where the existence of pulmonary emphysema was suspected. But in order to estimate the positive value of deformities of the chest, as signs of disease, M. Woillez saw, that the first step was to ascertain whether or not such deformities existed in the physiological state. In pursuance of this idea, the author examined and measured the chest of 116 individuals of the male sex, taken without regard to the affections which brought them to the hospital. In each case he noted the age, height, trade, existing and former diseases, health of parents, habitual state of the respiration and of the heart's action, terminating his inquiries by the mensuration of the thorax. The means employed to ensure freedom from error, in estimating its form and dimensions, were

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* Faust.

such as to remove all doubt of the correctness of the general propositions laid down in the work; they are, however, described with so much fullness, that we are obliged to refer for them to the original.

The mean age of the subjects examined was thirty-seven. None of them were affected with rickets, or extensive congenital malformation of the thorax; in no case was there œdematous infiltration of the thoracic parietes, or excessive general *embonpoint*. In sixty-seven subjects there was no thoracic disease; while forty-nine had, at the time of examination, some affection, most frequently pulmonary tubercles, or emphysema of the lungs, pleura, or heart.

The chest was well formed and symmetrical in *twenty-four* cases only; a proportion which appears, even making allowance for the number of subjects with disease of the chest, extremely small, and one which will no doubt startle those unused to careful inspection of its form.

The *partial* deformities observed consisted of prominences ordinarily slight in degree, but yet such as to be visible to the eyes, and of depressions of variable size. Our author examines them as they appeared in the various regions of the chest. His general results, for to these we must confine our extracts, are as follows:—"1st. Physiological prominences, of similar form to that of morbid ones, are of very frequent occurrence. 2ndly. They are almost always situated at the *left* side anteriorly, and the *right* posteriorly, frequently co-existing in these positions. 3rdly. They are exceedingly rare on the *right* side anteriorly, and on the *left* posteriorly, and appear never to co-exist in these positions. 4thly. In individuals whose chest appears well formed to the eye, the two sides are found unequal on mensuration; the *right* side is normally larger than the *left*. 5thly. In left-handed subjects, however, the left side is more developed than the right, which appears to depend invariably on hypertrophy of the left pectoralis major. 6thly. The measurements of the chest are modified by physiological and morbid prominences, but especially by the anterior ones; the posterior being occasionally accompanied by lateral curvature of the spine. 7thly. In subjects affected with continued fever of a certain standing, and, doubtless, in those suffering under old intermittent fever, the enlargement of the spleen modifies the chest's measurements."—(P. 51.)

We need scarcely impress on our readers the importance of these conclusions. The fourth contains information of the highest value for those who are in the habit of drawing inferences as to the state of the lungs, from a superficial glance at the form of the thorax; no doubt that, which the researches before us prove to be, in many cases, the normal condition of things has frequently been marked out as a morbid state. But the reader possibly doubts our author's assertion, and inclines to ask for his proofs. None can be stronger than those given—the accurate measurement of *one hundred and sixteen* thoraxes, undertaken without any preconceived notion on the question.

The proposition relative to the spleen is interesting, but we cannot help thinking that cases in which enlargement of that organ is sufficiently great to affect the chest's dimensions, must be very rare. Of this, at least, we are convinced from experience, that the first effect of such enlargement is to depress and drive to the right side the neighbouring abdominal viscera.

One of the chief difficulties, as it appears to us, in pursuing these inquiries, must have been the decision, in each case, of non-symmetrical formation, as to its being a physiological or morbid condition. Within the limits of a thesis it would have been impossible for our author to give those details which would alone furnish just grounds for decision; namely, an exact description of the state of the thoracic organs during the entire life of each subject. We must, therefore, be contented with his assurances of having carefully ascertained their condition in every individual, and, indeed, the general exactness of his facts and soberness of his inductions, are strong guarantees of his having done so.

The position of the nipples was non-symmetrical in eight subjects. In seven of them the right nipple was higher than the left; in the other, the reverse was the case; the difference in height varied from half an inch to an inch. None of these individuals had ever been affected with pleurisy. Of this remarkable peculiarity we have ourselves seen a good number of examples.

Previously to entering on the second part of his subject, the diagnostic value of the

alterations of form described in the first, M. Woillez discusses the question of the mode or mechanism by which these changes are effected. As his remarks bear materially on some of the conclusions in the work, and form the first attempt at a rational explanation of the appearances under consideration, we shall give a brief sketch of them.

The lung in the healthy state, and left to its own elasticity, fills only the third or fourth of each thoracic cavity. By means of its *extensibility*, however, when acted upon by atmospheric pressure, it fills the entire cavity, and is applied against the ribs. The elasticity of its tissue, called by our author its *concentric* force, constantly tends to bring it within a smaller compass, and consequently to produce a vacuum in the pleura.

The cavity of the chest is filled by the lung, as a chemical vase for collecting gases is by the liquid within it. In this case the water, instead of pressing against the walls of the vase, tends to escape from them; but the force which makes it do so not being sufficiently strong to overcome the pressure of the atmosphere, the liquid continues to adhere to the sides of the vessel. If the latter force be diminished, or balanced, the water, by reason of its weight, escapes from them. By substituting the cavity of the chest for the vase, the lung for the liquid, and the *concentric* force for the weight of the water, the truth of this comparison becomes evident. Hence, so long as the *concentric* force exists, no matter to how small a degree, the walls of the chest cannot be pressed against by the lung; it is, therefore, pure theory to consider the lungs to undergo pressure in a narrow chest; the truth is, that in this case, the ribs being less convex externally than in a wide thorax, the chest cannot expand so fully in inspiration.

"It results from the above, that the lung may be so increased in size as to exactly fill the cavity of the chest in the dead subject, (or, in other words, that it may reach double or treble its natural volume) without the corresponding side being in the least enlarged. But if the augmentation of the viscus exceed this point, the *concentric* force, and the tendency to a vacuum, cease to exist, while an *eccentric* force is produced which grows more and more considerable in proportion to the lung's increase in size. In this case the lung protrudes partially from the chest on the opening of the body, —the only case in which dilatation of the thorax by enlargement of the lung can possibly be conceived."—p. 31.

In cases of partial enlargement of the lung (as in emphysema), the destruction of the concentric force is partial also, and the walls of the chest acted on to the same extent only. The partial nature of the disease explains how, though local prominence of the chest be really produced, there may be no tendency in the corresponding part of the lung to protrude from the thorax.

In the case of thoracic effusion, the explanation is the same, with such slight variation as the reader's sagacity will easily supply.

It is plain that deformities of the chest must be produced by unusual development of the thoracic viscera, or by solid or fluid products in the serous membranes.

To begin with the organs, and of these with the lung:—Inflammation, hypertrophy, and emphysema, are the affections which may give rise to notable increase of size in that viscus. The question is, does that increase react on the walls of the chest?

In pneumonia the hepatised lung occasionally acquires double its natural size, and in consequence fills, or nearly so, one entire side of the thorax; but, as our author observes, no cases are on record in which protrusion of its substance took place on opening the chest after death. Now, we have shown that dilatation during life can only be admitted when such protrusion is observed; it consequently cannot occur in pneumonia, even when the entire lung is hepatised. The case is of course different if there exist any effusion into the pleura.

In examples of hypertrophied lung succeeding contraction of the opposite side of the chest, Laennec occasionally observed partial hernia of the lung after death. Our author's reasoning obliges us to admit, in these cases, dilatation of the thorax during life; but to prove it by direct observation would be a difficult task, as the contracted state of the opposite side deprives us of a standard of comparison for that which is believed to be enlarged.

The remarkable prominences produced by pulmonary emphysema have been made known to the profession in the recent work of M. Louis on that affection. According to that eminent observer, elevation of the super-mammary region exists in every case

of emphysema, more frequently on the *left* than on the *right* side, in the ratio of 23 to 11. Besides this, the natural hollow of the post-clavicular region becomes more or less completely effaced. With the general correctness of these statements, especially as regards the importance of external tumour in the diagnosis of the disease, M. Woillez coincides. But the fact already alluded to, namely, the much greater frequency of *physiological* prominences on the *left* than the *right* side, warns us not to conclude too readily that emphysema exists because the *left* super-mammary region is unusually developed. Such elevation, though less frequent on the *right* side, is *there* of much more diagnostic importance.

Sénac was the first to speak of the elevation of the precordial region as a result of hypertrophy of the heart, and to relate a case in confirmation of his assertion. Corvisart says that "the walls of the chest are sometimes more rounded and prominent than natural in individuals affected with diseases of the heart." Bouillaud has noted a vaulted shape of the cardiac region several times, "in consequence of *enormous* hypertrophy of the heart." M. Louis, too, informed our author that he had met with one similar case. Such a host of testimony proves, beyond question, the reality of the phenomenon. But if we recollect that it only exists when the heart's overgrowth is *enormous*, and, consequently, made evident by various other signs, its value, as an aid in diagnosis, wil appear very trifling. Besides, in seven cases in which M. Woillez detected prominence of the *entire* left side, but by far more marked at the *precordial region* than elsewhere, it was physiological, and unaccompanied, in every instance, with palpitations. The only distinctive sign of the normal and abnormal prominences would be the *stationary* character of the former, and the *progressive* increase of the latter—a minute distinction which could only very rarely be seized.

With regard to the tumours produced externally by aneurysms of the large trunks, authors generally speak only of those occurring at an advanced stage of the disease, when the neighbouring bone is worn away, and the nature of the case has long been matter of certainty; but, in truth, they produce an appreciable effect on the form of the chest at a much earlier period, and when the aid of such altered form in the diagnosis of the disease is of the first importance, if combined with dull sound on percussion. The tumour caused by aneurysmal growth is chiefly observed behind the cartilage of the right second true rib. We have ourselves seen two cases in which this prominence formed the most important element in the diagnosis of aneurysm of the arch of the aorta. In both the existence of the affection was proved by the autopsy.

M. Woillez distinguishes three stages in pleuritic effusions:—1st. Accumulation of liquid, without dilatation of the thorax; 2dly. Accumulation with dilatation; 3dly. Absorption of the fluid, and contraction of the corresponding side of the chest.

On this distinction he founds some novel and interesting considerations of which we regret our want of space prevents us from extracting the substance. Amongst the rest he offers an ingenious substitute for the very meagre explanation given by Laennec of the fact that the fluid in pleurisy, when secreted in a certain quantity, spreads in an equable manner over the entire surface of the lung instead of gravitating towards its base.

The effects of aeriform effusions into the pleura are next discussed, as well as those of solid formations in that membrane. Of the latter M. Woillez saw one remarkable example, namely, a collection of encephaloid matter dilating the chest to a very great extent.

Our author now passes to the pericardium; the elevation of the precordial region produced by serous effusion into that membrane was first noted by Louis in 1824. Since then its existence has been frequently noted by then observer; so constant is it, indeed, in its occurrence, that in thirty-two cases of pericarditis it was absent once only. This statement shows its importance in the diagnosis of an affection which so often baffles the acumen of the practitioner.

M. Woillez's essay terminates with a summary of its contents in the aphoristic form, and with two tables showing at a view the nature and frequency of the deformities observed.

We have given this extended notice of a thesis principally for the purpose of showing what may be done by a pupil under a liberal and well-directed system of medical education. It is in vain that we look for

similar productions from the pen of students in this country; but the fault is not theirs, and we hope soon to see the day when judicious institutions will produce the same fruits here that they do abroad.

INVESTIGATION, TRIALS, AND EXPLANATION OF ANIMAL MAGNETISM,

By Dr. Sigmond.

To the Editor of The Lancet.

Sir:—I have been induced to direct my attention to a subject which, after a lapse of many years, during which it was consigned to oblivion, has again started into notice and arrested the public consideration,—I mean animal magnetism. Had the bold and loud pretensions of those who first introduced it as a therapeutic agent into England, rested upon a solid basis, I do not think it could ever have been so completely rejected and ridiculed, as it very quickly was. It was received with the most extraordinary enthusiasm; the number of proselytes in the year 1790 was immense. A Mrs. Pratt, who published a list of cures performed by Mr. and Mrs. De Loutherbourg, and who, in her zeal for magnetism, applied the most extraordinary scriptural language to promote its belief, tells us, as a "lover of the Lamb of God," that three thousand persons, at one time, waited to gain admission to Mr. De Loutherbourg, when practising this art at Hammersmith. In the year 1785, Mesmer had imparted his doctrine to three hundred disciples, and one of them alone, Dr. D'Eslon, boasted that he had instructed, in one year, one hundred and sixty physicians. Notwithstanding this multiplication of magnetists, where are they now, and where the records of the benefit that society has derived from their labours? With this reflection, and with a determination to examine the works of Van Helmont, of Agrippa, of Baptista Porta, and of the authors whom the magnetists have commended as the depositories of their arcana, I entered the field of inquiry as a sceptic, and as such, after my inquiries, I remain, as to the belief that any individual is in possession of a power, save that which the strong mind exerts over the weak one, by which he can exercise a preternatural effect over the human frame. I totally disbelieve the existence of any fluid which can, at the will of an operator, be made to pass from his body into that of another, and thus, at his command, produce unwonted sensation.

In the course, however, of my imitations of the operations I have seen performed by others, I have observed certain most singular phenomena to arise, with which I was previously unacquainted, and which, I think, are of some value, and which, by the cautious investigation of the highly intellectual men who are constantly seeking to add to the means of prolonging life, and rendering it happier, may yet be found to lead to some physiological and psychological facts. I merely wish to state what I have observed, and to offer to show that certain consequences result from a peculiar kind of manipulation, which may easily be acquired, and which, if practised with dexterity, in some instances, might be productive of considerable influence in different conditions of the body.

Some weeks since the Baron Du Potet de Sennevoy, did me the honour to invite me to be present at a trial of his magnetic power at the *University College Hospital*. I there saw him perform a series of actions upon different individuals, and he, in two instances, produced what may be termed artificial sleep upon two females, and this was the full extent of his success; his other attempts were failures. The successful cases, however, arrested my attention; they seemed to be the result of simple means, nor could there, at least I thought, be a doubt that the same power existed in any individual who chose to exert it. This species of magnetism, I must observe, differed very materially from what I had seen practised many years since by a pupil of Dr. Maineduc, at Bath; it had less pretension, and was much more practicable. From the little opportunity I had of judging of the Baron Du Potet, I drew the conclusion in my own mind, that he felt an honest conviction that he was possessed of a peculiar faculty, and most distinctly do I acquit him of any intention of playing upon the credulity of those by whom he was surrounded; but I feel that I have a right, with the same candour, to say, that I do not think that he is aware how he produces the extraordinary effects that followed his manipulations.

I immediately determined to investigate the subject, and for that purpose tried a great number of experiments; but I was most unwilling, for a great length of time, to make my observations at all public, because I thought that I might be accused of seeking notoriety by investigating a subject which rather belonged to the community than to the profession, and one which seemed to be addressed to the popular feeling so easily excited, rather than to the calm and dispassionate consideration of the followers of science. Finding, however, that two distinguished members of the profession, Dr. Elliotson and Mr. Mayo, thought the subject worthy their attention, I persevered in my observations; and, added

to this, I was invited by an illustrious individual, whose regard for the medical profession, and for everything connected with it, I have, from my official position, had opportunities of witnessing and admiring. Earl Stanhope not only attended the Baron in his visits to the hospital, but devoted the energies of his highly-cultivated mind to an examination of the merits of the system, and he did me the honour to express his wish that I should prosecute the labours I had commenced. As a lecturer, too, I thought this a subject intimately connected with therapeutic powers, and as, by the publicity of your Journal, I am standing before the profession, I have felt that I am not intruding myself by giving the result of my experiments. These have been performed, of course, with the full certainty that I possessed no peculiar power which could be productive of any effect that might not attend upon the exertions of any other individual. The extent of my examination has been such as to satisfy me that I can produce a sleep of a very unusual character, by certain manipulations which do not require me to be in actual contact with the person upon whom the operation is intended to be performed; that I have acquired a certain degree of experience, by which I know how to accommodate the manipulations for the purpose required; and that I can communicate to another individual, in a short space of time, all the information necessary for the production of this sleep.

I commenced my series of experiments by imitating the actions of the Baron Du Potet. My first subjects were of the uneducated class; but I found them so prone to believe in the marvellous—so anxious for extraordinary results, that they deceived both themselves and me. I have since tried the same manipulations upon the higher classes, and though I find them much more sensitive to every impression, and their nervous system more easily acted upon, and although, occasionally, the imagination has led some of them away, yet I have succeeded in giving a very peculiar sleep, amounting almost to stupor, to a vast number of individuals. I have constantly found females much more susceptible of the influence than men; nor does it produce upon them all precisely the same state of sleep. For while in some it is a sort of trance, during which, as often occurs in that unnatural state, pain is scarcely felt, in others it produces hysteria, convulsions, and I have likewise known fainting occur. The most remarkable case that has fallen under my observation, and which, while it excited in me great anxiety, and the deepest interest, has taught me to prosecute my researches with extreme caution, has occurred to me within the last two days. I was enjoying the hospitality of a most amiable family in Fitzroy-square, when animal magnetism became the topic of conversation, and I related the trials I had already made. One of the young ladies proposed to become the subject of experiment, to which I very willingly assented; for, having on former occasions attended her during momentary sickness, I was fully aware of the natural strength of her constitution, and the absence of that nervous temperament which renders this system totally inapplicable. I began what are technically called "the passes." They, as is not unusual, excited laughter and incredulity. I proceeded for about five minutes, and then stopped and inquired if any sensation was produced, and the answer was, "a slight sleepiness;" and ridicule was again thrown upon the subject. I recommenced the manipulations; I observed the eyelids falling, and at last they closed; but, as the same incredulous smile remained, I persevered for three or four minutes, when I, almost doubting whether any influence had been produced, inquired what the feelings were; to this no answer was returned. I found my young friend was in the most complete trance I had ever yet witnessed as the result of my magnetism. The stupor was most profound; and I then tried the usual means to arouse her, but they were vainly exercised. After a few minutes I found the hands become icy cold, the face lost its natural hue, and became perfectly pallid; the extremities became quite cold; the respiration was imperceptible; the stimulus of light did not affect the eye; on speaking to her a faint smile was excited, and a quivering of the lower jaw, which seemed to indicate a wish but an incapability of answering; the pulse became gradually feebler, whilst the external appearance altogether bore such a decidedly deathly cast that naturally some apprehension was excited amongst her family, by whom she was surrounded. Of course I could not but feel a certain degree of anxiety and regret that I had produced such a state, and much uneasiness at the thought that I had inflicted a moment's alarm to my kind friends. These feelings were, however, less acute, from the full knowledge I entertained that the family had long reposed the most perfect confidence in me, and that no member of it had that nervous susceptibility, which would have embarrassed me had any untoward accident presented itself.

I placed the perfectly unconscious subject of this distressing scene in a horizontal position, and directed the application of warmth and of friction to the extremities. Circulation and animal heat were gradually excited, but she presented a most singular appearance of suspended animation. In this condition she remained more than four hours, for I had commenced a little after ten in the evening, and it was about half-past two, that, on some slight effort being made to rouse her, she uttered some of the most

piercing shrieks I have ever heard; there were convulsive efforts to raise the limbs; the face, too, became convulsed; she opened her eyes and stared wildly around; she was placed in the upright posture, and seemed sensible. Advantage was taken of this circumstance to carry her to her apartment; before, however, she could reach it, she fell into a profound slumber, but its character was more natural. She was placed in her bed, appearing perfectly composed; the countenance had acquired its natural hue; the respiration was perfectly easy, and the pulse natural. In this state she remained during the whole of the day, until nine o'clock in the evening, once only opening her eyes, and addressing a few words to an anxious and affectionate sister who never left her side. In the evening the young lady joined her family perfectly restored to her wonted cheerfulness. She expressed no complaint whatever. She stated that the feelings that first came over her were those of extreme quiet and repose,—a species of ecstasy,—a gradual languor seemed to steal over her; that she heard something passing around her; felt an inclination, but an utter impossibility, to reply. The first waking up she, however, described as almost terrific. It was as if she was bursting from a narrow and confined space, and as if she arose from interminable darkness. The lesson that I have thus learnt will not be lost upon me.

It is upon the respiration that my efforts are directed, and the principle is precisely that which is called "stealing the breath away;" besides which, by the undulations of the air caused by the movements of the hands an unusual cold is produced which very much assists the effect. The art seems to me to consist in obliging the individual again to inspire, by the nostril, the carbon he has already expired, whilst the currents of air caused by the extended fingers produce some effect upon the facial nerves, thus inducing the eyelids to fall down. Association of ideas is very strikingly evinced by those who have been once magnetised, for then the slightest quantity of manipulation is necessary; the patient evidently having acquired a habit of inhalation by the nostrils. It is from the centre of the nose downwards that the effect is most speedily induced, and the drawing of the hand downwards from the brow, so as to effect the eyes, I find to be quite unnecessary towards producing the effect. I do not consider the process I have employed by any means perfect; and I have little doubt, when the attention of the profession is drawn to the subject, that considerable improvement may result; at the same time I am persuaded that the manipulations may be the cause of very great alarm. I have now exercised this art upon nearly a hundred persons, and with very general success in the fairer part of creation; I have quieted delirium and given sleep where it has been for many nights vainly solicited. I have magnetised in the presence of many medical men who have been in attendance on the Baron Du Potet's lectures, and they have declared that the sleep appears identical with that he produces, and that it is proved by the fact that animals may be sent to sleep by the same movements. I am very anxious that the members of the profession should try the same process. There are some curious histories on record, which plainly prove that this mode of producing sleep and of mitigating disease, was known, and that demons were supposed to be cast out by such manipulations, but they were ascribed to the operator's wearing a charm, as was the case in the instance mentioned by Josephus, where, in the presence of Vespasian, his sons, and the Tribunes, a man possessed of a demon suddenly fell down, after the ring had, whilst he was breathing, been waved near his nostrils. I think the subject very well worthy investigation, and that it may throw some light upon the phenomena observable during sleep. I have the honour to be, Sir, your obedient servant,

G. G. SIGMOND.

Tuesday.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, Nov. 28, 1837.

President, Dr. BRIGHT.

REMOVAL OF THE CLAVICLE WITH A TUMOUR SITUATED IN THAT BONE.

By BENJAMIN TRAVERS, *F.R.S., Senior Surgeon to St. Thomas's Hospital.*

THE patient in this case was a young gentleman, aged 10, a native of the East Indies, but resident, for the last five years, in England. In the summer of 1836, a swelling, the size of a hedge-nut, was observed in the centre of the left clavicle, which was attributed to a fall he had ten days previously, and treated accordingly. At about two months after this discovery, the author was consulted, when the swelling had attained the size of a pigeon's egg, was elastic and painful when compression was made on it. Various means were had recourse to, with the view of dispersing the tumour, but without avail, as in the month of May, in the present year, its base from the scapular extremity occupied three-fourths of the bone. About two-thirds of its bulk projected above the clavicle, so that in the erect posture it was seen from behind. It being determined to remove the bone from the tumour, the operation was performed on the 6th of June, 1837, twelve months after the commencement of the disease.

The operation was effected without difficulty, and no considerable blood-vessels were injured. The entire clavicle was removed, with the exception of a small portion of the sternal extremity. At the date of the present narrative (Oct. 1837), the wound had been long completely cicatrised; there was no falling forward of the shoulder, nor any restriction of the motions of the arm; indeed, such facilities does the patient enjoy in this respect, that one of his amusements is the rowing of a boat on the Thames. There is a production of bone from the truncated extremity of the clavicle, extending at least two inches, and terminating beneath the centre of the cicatrix. A section of the tumour, in its longest diameter, presented an arrangement of cells filled with dark, solid coagula of blood. The author conceives that there can be no reasonable doubt of two points; namely, that the tumour began in the cellular structure, and that it was a result, direct or indirect, of the fall. The explanation offered by the author, is, that a medullary extravasation had taken place from the concussion attending the fall, or from the fracture within the periosteum; in either of which cases, the effused blood which, in a perfect solution of continuity, would have been absorbed, acted as a foreign body, and, by effectually stopping the osseous secretion and starving the bone, became the instrument of the absorbing process. He is induced, by the phenomena of such cases as the present, to entertain an opinion that many anomalous tumours on record arise from changes within the medullary canal of the bones, and are exasperated into intractability by their confined situation. The paper concludes with a categorical summary of the diseases of bone which the author's observation and the records of surgery enable him to verify.

Mr. Arnott did not feel satisfied with the pathology of the case as given by Mr. Travers. He remembered the case of a man, 65 years of age, which was somewhat analogous. In this instance the patient fractured his clavicle, shortly afterwards a swelling appeared over the fractured part; the sternum soon after presented a similar kind of tumour. These tumours were not painful, but elastic and pulsating; the pulsation, however, seemed to be rather communicated to, than existing in, them. Two months before death the patient complained of a severe pain in one of his thighs, on examining which no cause for the pain was detected. It was considered that a tumour was forming in the lumbar region. The pain in the thigh was most excruciating, came on in fits, and was not relieved by opiates. After death a tumour was found on that side of the loins corresponding with the affected thigh. A tumour, about the size of an egg, was situated in the clavicle; the periosteum was entire and covered a mass of damson-coloured blood divided into several departments. A section of the tumour of the sternum presented the same appearances. In another case in which he (Mr. Arnott) removed the arm for a tumour covered by periosteum, the patient remained well for three years, but the disease afterwards recurred in the skull. On examination, this tumour was found to extend into the diploe, another tumour of similar character was about being formed on the opposite side of the head, and, on making sections, he found similar tumours in the clavicle of each side, also in the thigh bones, sternum, and some of the ribs. The tumours are situated in the cancellated structure.

Mr. Macilwain considered that the injury alone, unless there was some constitutional diathesis present, was insufficient to account for the occurrence of the tumour.

Mr. Hawkins thought there must have been some peculiarity in the constitution of Mr. Travers's patient; the boy being healthy before the accident, and still continuing well after the removal of the tumour, was no proof that it was not malignant. Persons, after the removal of malignant disease, might live many years free from a recurrence of the affection, but eventually perished from its return. Reasoning from other cases in which extravasation of blood followed injuries of bone, he thought Mr. Travers's explanation not satisfactory.

Mr. Earle believed the disease would ultimately prove to be malignant. He remembered a similar kind of tumour following a sprain of the wrist. He considered, at the time, the tumour was malignant, but owing to the great pain the patient suffered, he was induced to amputate the limb; the disease returned, and the patient died. In this case the tumour presented much of the appearance observed in Mr. Travers's drawing. He considered the simple extravasation of blood following the fracture, was insufficient to account for the disease in Mr. Travers's case, and he related two cases of fractured tibia in which the periosteum was entire, and there was much extravasation between the divided portions of the bone. The blood remained fluid for some months; in one case puncturing the part, and letting out the fluid blood, followed by compression, brought on inflammatory action, and union followed. In the other case, in which the fracture was oblique, an elastic gum bougie was placed in the line of the fracture, surrounded by pasteboard splints; the bone united. He considered the blood, in these cases, prevented union simply by acting as a foreign body.

Mr. Travers, looking at all the circumstances of his case, could not believe it was malignant. The healthy state and youth of the patient, the fracture or bruise of the clavicle, from which he suffered much pain

at the time—the tumour appearing in ten days after the accident, and gradually increasing in size—the quick recovery of the patient after the operation, and his present good state of health, were against the opinion of the tumour being malignant. He thought it was quite philosophical to look for a simple cause, where we could find it, for an effect; and the explanation he had given of the cause of this tumour, was satisfactory to his own mind. The cases related, though in some points similar, in others differed from the one he had brought before the Society.

At the meeting on December 12th, will be read an abstract of a Report of twenty-one cases of cholera which occurred in the *Seaman's Hospital Ship*, in the interval between the 8th and 28th of October, 1837, by George Budd, M.D., Physician, and George Busk, Esq., Surgeon to the *Dreadnought*. This paper is looked forward to with interest.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Dec. 2, 1837.

Mr. Hale Thomson, President.

BRONCHIAL POLYPI.

Dr. Granville stated that, at the next meeting, the Committee which had been formed for the purpose of investigating the subject of arsenic in candles, would lay their Report before the Society. Dr. G. also stated that his observation on eunuchs at the last meeting, referred only to what he had observed whilst abroad.

Dr. Scott brought forward a motion to the effect, that a deputation from the Society be formed for the purpose of communicating with the Secretary of State respecting poisonous candles. The motion, however, was withdrawn at the suggestion of several of the members, who considered the report of the Committee should precede any such proceeding.

Mr. North laid before the Society two very fine specimens of bronchial polypi.* The subjects from which these preparations were obtained were males, both of whom had suffered from chronic affection of the chest, unattended, however, with any symptoms of severity, except during the attacks of dyspnœa which immediately preceded or accompanied the expectoration of the morbid growths. In one instance, twenty or thirty of these substances were expectorated during the course of one week. Mr. North said that two classes of bronchial polypi existed. In one, hæmoptysis precedes the expectoration of the growths; in this class the fibrine of the blood may assume the shape of the bronchial ramifications. This form of the disease is much more fatal than the class to which the specimens exhibited belong. These may, or may not, be connected with chronic bronchitis, and are only dangerous in consequence of the violent coughing and dyspnœa, which, in some cases recorded by Andral and others, produced suffocation. The treatment which had been found most efficacious in these cases, was the inhalation of steam from hot-water. In connection with the general indications, bleeding was not necessary in either of these cases; emetics, in one instance, were tried, but without benefit.

Dr. Williams referred to the analogy which existed between this disease and the croup of infants. A late writer had noticed a disease something like this, in connection with peripneumonia. In similar cases, where the fauces and pharynx contained a similar kind of secretion, astringents locally applied, removed the deposition. Bleeding, he considered, was not required, except in those cases in which there was no disease of the heart present; and when plethora or congestion produced great dyspnœa. Emetics, in some instances, would act as beneficially as in suffocative catarrh. Alkaline attenuants would also be useful, alkalies, as was well known, having the power of rendering the secretions less viscid. In one instance after death, this kind of polypus existed in some of the bronchi, and pus was in contact with them.

Dr. A. T. Thomson considered emetics in this affection would be useful in two ways; first, on account of their mechanical action; and, secondly, by their physiological influence in promoting the secretion of the bronchial mucous surface.

Dr. Jewell related a case of a young lady, by profession a singer, who was subject to the expectoration of these morbid concretions. She was ultimately obliged to relinquish her mode of life, after which the affection never returned.

The President noticed that a patient of his had told him that something membranous had been torn out of his urethra, probably a substance analogous to the growths under consideration. Mr. North and Dr. Williams had observed analogous depositions, from the mucous lining of the intestinal tube, after drastic purgatives, and in the uterus after death.

Mr. Hardwicke stated that Dr. Elliotson was frequently subject to the expectoration of bronchial polypi. In general, Dr. E. did not consider such occurrences attended with any danger.

* Mr. North took occasion to mention the excellent manner in which Mr. Miller puts up pathological specimens, and referred to the beautiful preparations in ovology which he possesses.

BRITISH MEDICAL ASSOCIATION.

Tuesday, Dec. 5.

A MEETING of the Poor Law Committee was held this evening at *Exeter Hall*, when various communications respecting the medical attendance on the poor were read and discussed. A communication was received from Mr. Wakley, stating that he had succeeded in postponing the Parliamentary inquiry into this branch of the Poor Law until after the Christmas recess. It will be brought on, however, immediately afterwards.

ALLEGED NEGLECT OF THE SICK-POOR IN THE KINGTON UNION.

To the Editor of THE LANCET.

SIR:—Referring to the article headed "Treatment of the Sick-Poor under the Medical Union Contract System," in page 278 of your publication of the 18th ult., I have requested the bearer, my agent, to call upon you for the original article and letter of "Humanitas," with the private note referred to by you.

My agent will inform you that I have been instructed, by the "medical officer" alluded to, to institute immediate proceedings in reference to that article.

With these instructions before me, I feel that it would be as improper as imprudent to offer any detailed account in contradiction until the result of the inquiry about to be instituted be ascertained; but, in the meantime, upon the entire faith which, from a previous knowledge of the character of the officer in question, I have not hesitated to give to the statement made by him to me, I feel it but due to that officer to state, that the publication of "Humanitas" is not only wholly destitute of truth, but that it is composed, from beginning to end, of wicked and malicious falsehoods. I am, Sir, your most obedient servant,

RICHARD SILL.

Castle-street, Hereford, Dec. 4, 1837.

*** We insert this letter without hesitation, and, whatever may be the result of the investigation, it shall, in due time, be made known to our readers.

WORKS RECEIVED.

On Diseases of the Rectum; by James Syme, Professor of Clinical Surgery in the University of Edinburgh. Longman and Co., London, 1837. 8vo. pp. 138.

Remarks on the Treatment of Fractures of the Lower Extremities without the Use of Splints, with Cases; by John F. Burke. Highley, London, 1837. [A Pamphlet which contains some judicious observations on this method of treating fractures.]

The Retrospective Address of Dr. James L. Bardsley to the Provincial Medical, and Surgical Association. Worcester, 1837.

Portrait of Dr. J. Müller, Professor of Anatomy and Physiology at the University of Berlin. Schloss, 1837. [This is an excellent and well-finished likeness of Professor Müller.]

Reports of the Directors of the Montrose Lunatic Asylum, Infirmary, and Dispensary, for the years 1836 and 1837.

The Phrenological Journal, New Series, No. I. Simpkin, Marshall, and Co., London.

TO CORRESPONDENTS.

Mr. Radley's paper has been received,—it shall be inserted, in part, if not in whole.

A correspondent signing himself "*Un Médecin Malgré lui*," complains of a practice, still carried on in some hospitals, of surgeons operating in private. He also alludes to a clinical professor of medicine who fabricates cases, upon which he lectures, for the purpose, we suppose, of avoiding the trouble of observing real cases in the wards. If our correspondent will speak more precisely, and authenticate his statements, we will publish them with pleasure. Proceedings of the kind alluded to by our correspondent well merit exposure.

A reform of medical law, which would effectually accomplish the objects described in the letter of *Mr. Edward Heran*, has over and over again been advocated in the pages of this Journal. Mr. Heran does not propound anything that is new, or we would certainly, with great pleasure, insert his communication. Our volumes teem with illustrations of the necessity and propriety of medical reform in Ireland as well as in England.

Such speculations as those of *Mr. E. E.*, on life, closing in a surmise that "blood in motion is life;" and on the seat of the soul, that "its residence is taken up throughout the whole body, in the same way as the blood permeates the frame," cannot be published with profit, because they are based on conjectural facts, which are themselves liable to interminable disputes.

The description of the key instrument for extracting teeth, could only be made clear to readers by the addition of an engraving. The protection of the instrument, however, by a *patent*, renders it a subject for our advertising columns, where its mode of employment and usefulness may be properly and effectually illustrated.